ADMINISTERING RESEARCH AND DEVELOPMENT

The Behavior of Scientists and Engineers in Organizations

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ADMINISTERING RESEARCH AND DEVELOPMENT

The Behavior of Scientists and Engineers in Organizations

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PREFACE

We conclude our work on this book with mixed feelings: relief over the freedom from its undiminishing demands on our time for five years past, AND regret that we are terminating the experiences, the associations and the stimulus it provided. A moment's chat among ourselves, however, displaced those feelings and replaced them with new ones—equally mixed!

Conscious as we are that the thirty-six cases on the management of research and development groups toilsomely collected and printed herein constitute the first volume of clinical reports on administration "as she is" today in that swiftly growing sphere of management, we are equally conscious of the gaps in our collection. To give one example: it is disconcerting, on the eve of publication, to be aware that we must go before our publics and acknowledge that we do not yet have a case that centers wholly on the bothersome problem of what to do about the increasing professional obsolesence of technical and scientific personnel. It is coming to affect us all, yet with them it is rapidly becoming acute.

We also are reminded, frequently, by lively members of the student groups who have shared with us five seminars sponsored by the Industrial Research Institute, of the endless range of problems *they* want included. Beyond them there are other volunteers who propose additional cases they desire to have included. To all of them we have had to say, "Give us time; we hope to get into that issue as soon as possible."

Thus as we were congratulating, *and* condoling, among ourselves while taking leave of this book we began to realize we faced some fairly sizable unfinished business. Not only must we plan a revision of this book to better round out its coverage, but beyond it or even along with it loom additional books of cases largely because thirty-six cases cannot adequately reflect either the range of problems in the sphere of research and development, nor represent the rapidly increasing attention it is attracting from research workers in administration and organization. Whereupon we found ourselves victims of a swift transference of feelings: relief now that we were not to lose our stimulus, our privilege of exploration of important phenomenon, AND regret now that the pencils we relinquished yesterday are today back again in our fingers. It *is* a day of rapid change.

We are grateful, for ourselves, and also on behalf of users of this course book, for the generosity of those who have allowed us to include their research or conceptual contributions. We are especially indebted to the work of Dr. Herbert A. Shepard and Dr. Lowell W. Steele for intellectual insights we found quickly applicable in our case gathering.

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When combined with clinical data contained in the cases, their findings and formulations suggest that we may be reaching a threshold for establishing a successful science as defined by the late Elton Mayo.

> Science did not begin with elaborate and overwhelming systems, and thence proceed to study of the facts. . . . Scientific method has two parts, represented in medicine by the clinic and the laboratory. The two are interdependent, the one unfruitful without the other. The characteristic of the clinic is careful and patient attention to a complex situation any part of which may suddenly discover unanticipated importance; that of the laboratory is experiment and logical construction . . . the origin of science in firsthand observation may not be forgotten without consequence in experimental futility, illustrations of which may be seen all about us.*

Turning to others who have aided us we owe thanks to former Dean Stanley F. Teele who took an early interest in enlarging our work toward understanding better the special problems of administering research and development divisions, and took steps to put some of the School's facilities at the disposal of the I.R.I. seminars. Dean George P. Baker has continued the same encouragement and in other ways made evident his keen interest in a group, such as the Industrial Research Institute that has been sufficiently alert to the immediate practical value of research as to allot annually a sum that supports a substantial fraction of the costs for gathering new cases on research and development management.

The men who did much of the field work for us—James W. Stratton, Gerald C. Leader, and Andre Ruedi deserve credit for the accurate detail, the revealing episodes of behavior, the constant effort to be impartial and objective in their observations, and their enthusiasm for work that was often tedious and burdensome. In addition to the thanks we owe them, we owe special thanks to our friend and colleague, Professor Ralph M. Hower for his readiness to share the work—generally in the form of many cases that he produced while working on a project that resulted in the book, *Managers and Scientists*, referred to elsewhere in this book. Lacking his generous assistance, the appearance of the present book would have been much delayed.

We owe, of course, our appreciation to other colleagues here for their participation in the seminars, specifically to Professor Renato Tagiuri, who, in addition, has allowed us to include a version of the results of his research into the value orientations of managers and scientists, which is to be published at an early date, and also to Professor Alva F. Kindall, who has contributed to our group his wide and life-long experience in the field of Personnel Relations. William P. Gormbley, Jr., now with the Ford Foundation, was an active and valuable member of the group here while we were working through the creeping and the toddling stages of the ventures that have produced this book. His competent assist-

* Elton Mayo, *The Social Problems of an Industrial Civilization* (Boston: Harvard Graduate School of Business Administration, 1945), pp. 18–19.

ance with the Industrial Research Institute Seminar freed much other time for case research.

Since a great deal of the original impetus for the whole undertaking was generated in the Industrial Research Institute, it should be in the record that Dr. Robert W. Cairns, Vice President and Director of Research for Hercules Powder, and Mr. Don P. Krotz, Vice President in charge of Staffs Services Department of California Research Corporation, were leaders then in helping launch our joint enterprise. Their interest has continued through the years since as succeeding committee members have carried the load of liaison between our respective groups. Others who have given notably of their time and thought include R. H. Lueck, J. William Zabor, John R. Brown, Jr., and Robert Donahue. Backstopping the efforts of all the foregoing officers has been, and is, C. G. Worthington, long-time Secretary-Treasurer of the Institute.

Last, but emphatically not least, the three editors and authors, must warmly thank some members of the Irwin publishing organization. To Mr. Harry H. Bingham, Assistant to the Chairman, our thanks for a heavy chore of editing discharged so expertly that, at this end, it was virtually painless. To Norman Dorian, Eastern Regional Vice President, our appreciation for a highly business-like discharge of all his multitudinous duties, yet withal so patient he seemed deceptively casual. And to Richard Irwin, the acumen, the foresight that visualized this book before it was a discernible gleam in our eyes. He returned to that idea so unfailingly that it could be called pertinacity, yet it was so presented that it seemed the most natural, inevitable idea in sight.

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BOSTON, MASS.
July, 1964

Case material of the Harvard Graduate School of Business Administration is made possible by the cooperation of business firms who may wish to remain anonymous by having names, quantities and other identifying details disguised while maintaining basic relationships. Cases are prepared as the basis for class discussion rather than to illustrate either effective or ineffective handling of administrative situations.

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General Introduction

THIS COURSE BOOK has been put together to contribute to the better management of scientists, engineers, and technicians (of all professionally trained personnel, in fact) wherever they are, but especially if they are members of a research and development organization. It has been in preparation more than six years and is only now available because it has taken that long to collect trustworthy data on how research and development currently was *in fact* being managed. The thirty-six cases presented hereafter reveal what now *is* being done, what problems are the most prominent, and what resources of skill and knowledge are employed against them. These cases display, with clinical directness, the range of administrative response to the specialized situation of obtaining scientific and technical productivity from professionally oriented men in an industrial environment.

These problems are relatively new. Before 1920 they were virtually unknown. At that time scientists off campus were nearly unheard of, while scientists on campus fell into small clusters of four or five specialists with virtually nothing to manage beyond a small amount of inexpensive equipment and their own free time. Those days of now-nostalgic myth abruptly vanished forever with the advent of World War II and the Manhattan Project. Given overriding priority by the military race to perfect the atomic bomb, the Manhattan Project mobilized scientists from all fields, as well as engineers and technicians, on a colossal and unprecedented scale. Once mobilized they had to be organized and managed. Somehow they were, and a new era for science began, merely dramatized by the Manhattan Project, for off-campus scientists and engineers had been significantly growing before 1940 and by the end of the war were commonplace. Today science is Big Business with a total national budget over $20 billion and nearly all the problems that beset Big Business, particularly and especially those of organization and management. Scientific activity as a "cottage industry" will exist left over as a forgotten fragment from the time before enterprises were organized that now operate cyclotrons, space centers, International Geophysical years, N.A.S.A.'s, A.E.C.'s, etc.

The need for knowledge about research and development management is not only new—it is urgent. Business is becoming rapidly professionalized, largely through its increasing dependence on scientific and technical personnel needed to handle those operations. The blue-collar contin-

gent employed in American business peaked some years ago. The white-collar clerical, which has experienced a surge forward, will decline sharply as automation takes ever greater effect. But the ratio of scientific and technical personnel, which has been increasing steadily year after year, will continue and increase at rates that already stand in some specialties such as engineering at three and four times the average for other elements in business. Though firm figures are hard to come by, the impression is strong that before another decade passes, the majority of management as well as staff positions in American Big Business will be occupied by men with scientific and technical training.

As science now must learn to organize and manage within its own swollen numbers, so business equally needs to learn how to organize and manage recruits relatively new to its ranks. It is in the best interests of both parties, and of the nation that supports them, that these lessons be acquired as soon as possible, for the rewards and penalities that will accrue to the country which can meet this double challenge will be ones of great magnitude and long duration.

Out of the ferment generated by these tides of change affecting business as well as science many men and groups begin to grope for ways to meet them. One such group came to life in an association formed by the research departments of about 175 American industrial corporations that were actively promoting research in expectation of technological innovation and corporate profitability. The association took the name of the Industrial Research Institute and the group referred to formed itself to seek ways and means to a better management of their own research and development divisions. In time they were led to our School of Business Administration to see what we could offer. We would offer a good deal of case material on general management problems, viz: those that any manager in any organization could expect to encounter. Since we were convinced that a significant portion of every research director's job would resemble every nonresearch division manager's job, that would be some help. But we also discovered that the Business School case files carried very few cases drawn directly from research and development operations. So an agreement was arrived at whereby we would begin with what we had and send out research men to gather research and development cases. The agreement has been adhered to loyally, and half the cases in this book represent their contribution to this joint venture in management development. That has put us in a position to publish earlier the beginning of what we hope will continue and grow as a movement to apply research-by-cases into how research and development is organized and managed today in our country and, as a corollary, how it might be better organized and managed. The most important by-product may, in time, prove to be that whatever is truly *unique* to the management of research and development can be confidently distinguished from what it shares with management in general. These are some of the issues that can be expected to

come to light as the cases in this course book are discussed in knowledgeable groups.

Cases are, of course, the heart of the book, but they have been supplemented by a number of research reports that will suggest uniformities or limited generalities to be taken into consideration by an administrator seeking, as he must, ever-better alternatives. There are also conceptual papers which challenge existing conventions of management, or present new hypotheses to replace inadequate ones, or marshal facts that disclose new trends soon to impinge importantly on research and development organizations. We believe this tripartite scheme of arrangement (more fully described in Chapter 1) will contribute depth and illumination to the lessons that good discussions can distill from the cases. But it is from the cases, and the vigorous discussion thereof, that most can surely be gained so, at this point, a few observations on the case discussion method may enable the most to be made of this course book.

Scientists, although they may not recognize it, are old hands at the case method. Their earliest training, when it is more than mere rote, quickly confronts them with raw data as in our case material. Therewith they begin long years of practice in observing closely and acutely, of making finer and more salient discriminations, of essaying tentative and carefully limited generalizations to be tested, verified, or modified and replicated. As this daily drill proceeds, a student becomes increasingly conscious of the limitations of his data, of its sources and their shortcomings. He is expected to search for more data and better data, but is also expected to know what more data he needs, in which way the data is better, and how it will help him if he gets it. Often, of course, he discovers in this process that he has failed badly to utilize purposefully the data he has got. He must face the cost in time and effort needed to obtain additional data and to justify the expenditure. He learns too, as he goes, that overabundant data more often is a hindering perplexity than data seemingly too scanty. Ultimately, too, he learns that his boundless appetite for more data often masks a secret hope that if he but accumulates enough data an answer, "the solution," or a great discovery will yield itself without a tormenting creative struggle on his part with the inert material so laboriously collected.

Scientists in their training not merely learn to cope with recalcitrant data but do so in informal small-group settings, notwithstanding the fact that this is not consciously planned for or formally recognized. The most familiar scene in science is a group clustered around a lab bench, or at a professor's desk, or taking turns with chalk at some blackboard. Discussion is a constant accompaniment of scientific training and, if anything, is more prevalent after academic graduation than before. Furthermore, contrary to the stereotypes of the antisocial recluse, the eccentric tinkerer, or of the abstruse speculative meta-mathematician inured in a Spartan cell, most of the practicing scientists I have observed are exceedingly articulate and frequently argumentative men—easy, ready, and persuasive talkers

and more at home in animated, vigorous, and well-reasoned discussion than most other professional groups. The explanation for this is that behind all this familiarity and competence stands one of the key values in Western science: that truth will prevail if communication is unfettered.

We are happy to reveal this unexpected, but welcome, discovery that scientific training enables its graduates to benefit so readily from the discussion of problems in organization and management. They seem to comprehend, too, that continuing discussion of case after case will affect and alter their own managerial behavior. Rather than resisting this, they appear to welcome thorough discussion as a natural and workable method of converting greater knowledge into greater competence and better performance. This amounts to saying that we now are confident that men struggling toward a better administration of technically trained professionals have been unwittingly well equipped to benefit immediately from good discussion about the actual behavior of real people in living organizations. This is an unforeseen dividend from years of vigorous group discussion about any and every issue bearing on their pursuits in the physical sciences.

Today, all that seems needed is a conscious acknowledgment that their long years of drill in dealing with concrete physical data are applicable to dealing with another kind of concrete data, namely, with human behavior in organizations. The applicability goes further, too, because free discussion in this new instance will help, as before, to determine the true, not the superficial, nature of the problem; to subject to debate the evident alternatives and to evoke new and untried ones; to run up rough estimates on the risks and costs as against the rewards to be anticipated from each line of attack. And to become accustomed in administration, as now they are in research, to a continuing succession of new cases, each with helpful similarities to those that have gone before, yet each always with some unique aspect of its own inviting special consideration.

There awaits one more discovery for men trained in science when they commit themselves to a course consisting of cases concerning the administration of a research and development group. Assuming capable and earnest discussion, the moment will come, and all too quickly, when the existing beliefs, theories, and practices of management generally prevalent today will disclose a well-nigh total inapplicability to the management of research and development departments. It is an inadequacy nearly at its bankrupt worst in dealing with professional personnel.

Historically, the explanation for this state of affairs is simple. The conventions of management prevalent today were worked out during more than a century past while dealing almost exclusively with the blue-collar manual worker. From the Civil War to World War I, the overwhelming majority of the population at work in formal organizations consisted of unskilled "hands" busy at physical labor. We have inherited management codes and attitudes, beliefs, and practices formulated under circumstances

which have been widely and rapidly changing. It is still startling to be reminded again how dramatically and drastically the modern administrator's world has changed. In only a little more than one generation a wholly new class of employees has emerged as the element most indispensable to the success and survival of modern American corporations. Yet, in theory even more than in practice, we are largely oblivious of this massive displacement in our working population of brawn by brains, of hands by heads. We are only partially and confusedly aware that these new people bring with them new and deeply different needs, that their demands on organizations are different in kind, not simply in degree, and that we are fumbling largely in the dark as we try to insure their motivation, or to "manage" the creative talents we have hired them to contribute.

We need to acknowledge our hampering unpreparedness and to face the fact that a revolution in management theory and practice is inevitable, given the displacement of physical labor by professional specialists. Since there seems to be little option in the face of an evolution that cannot be halted or reversed, we might as well welcome our necessity as an opportunity to reconsider and re-evaluate what we mean by organization and management. If we must view, at last and in all earnestness, our human resource as the one in which the potential for productivity is the greatest —as unquestionably the commitment of capital is becoming the greatest —then the way may be cleared for a task of reconstruction of the relationships between men that could be the greatest achievement of this century. It must, in fact, rise to an accomplishment on that scale of magnitude or society will not be able to master—to manage, if you prefer—the materialism our ingenuity has released upon us.

Thus to those now preparing to better manage and administer, you are invited, indeed you are compelled, willy-nilly, to help create a more adequate conceptual world from which to organize and manage. And, because it will be partly of your own devising, it will serve you better than the one inherited from a past that now is vanishing—first in America, thereafter elsewhere in other industrial societies. Once again, perforce, we find ourselves pioneers skirmishing to establish little settlements as footholds on the long seacoast of a little known continent inviting exploration and occupation that it may yield its resources to the better organization and management of men.

J. C. B.

SECTION I

Introduction

Chapter 1

THE CASE METHOD AND STUDY OF MANAGEMENT (R & D)

THIS book has been designed to present to the reader three different but complementary kinds of data. On the one hand, in the cases (Sections I, III, and V) he will find clinical descriptions of actual problems and events which have concerned managers, scientists, engineers, and technical people working in research and development laboratories. In addition to and supporting these cases there are two types of readings. In Section II we have gathered several significant research studies. The conclusions reported are the result of research projects undertaken by the authors, and these reports of *facts* about R & D activities are transmitted to the readers. Finally, in Section IV the reader will find a number of conceptual papers. The authors of these papers, on the basis of considerable practical experience with R & D activities, and/or considerable knowledge of research studies which have been reported in the field of R & D management, have tried to pull together their *ideas* about why things happen as they do.

The case material may be approached in two ways: the cases may be *read* as bodies of clinical data which would be of interest to anyone concerned with problems of managing research of development activities, or they may be used as subjects for *discussion*, usually in the classroom, between people who are interested in becoming more skillful managers. Both the research studies and the conceptual papers are, of course, open to discussion and interpretation.

In putting together this volume we have quite obviously been selective in choosing the particular research reports and readings to be reproduced. We have, for instance, chosen only material which deals with human problems in research and development organizations—as contrasted with problems of evaluating research results, controlling research expenditures, or deciding upon what research to do. Further, from the vast body of literature on the human problems of R & D management we have selected those items which appear to us to be most useful within the context of the case material included herein.

Study of both the research reports and conceptual material should convince the reader that the various authors do not always agree. We might, in fact, have selected other conceptual readings where the differ-

ences in interpretation are even more obvious than in those reprinted here. The criteria for selecting the particular readings which will be found in this book have been that the points of view expressed appear (1) to be helpful to the manager who is struggling to understand the total situation he deals with in the research and development laboratory, and (2) they appear to bear upon and help illuminate some of the issues and problems which crop up in the cases.

The book has been designed in such a way that readers, instructors, or students can choose the particular sequence of cases and readings which appears most stimulating and useful to them. Neither the cases or the readings are in any particular order except that the introductory cases (Section I) deal with somewhat simpler data than the cases which follow.

The case method of study has been used for many years as a means of analyzing problems and developing the skills of participants. Courses in organizational behavior, administrative practices, and business policy at the Harvard Business School and elsewhere have benefitted considerably from this method of study. Much of the literature and many of the cases which have developed around such courses are revelant to the problems encountered by all levels of management in research and development laboratories. Why then this book, which focuses specifically on the administration of R & D?

If it is clear that the problems encountered in administering the affairs of professional technical people in organizational situations are in many ways identical with the problems encountered by managers of any organizational component, it is also apparent to anyone who has had experience in general management, as well as in the management of R & D activities, that the latter situations sometimes present very special problems which, either in degree or kind, are different or unique. The cases and readings in this book, therefore, have been selected to highlight the differences in issues which appear to characterize the management of R & D when these are compared with problems of general management.

Using This Book

It may be useful to "walk through" the book as a way of highlighting the unique aspects of problems which present themselves in R & D organizations—as contrasted with those found in other organizational components. Such a "tour" will also allow us to make a few suggestions about how the material in the various sections of the book may be used.

Having said that much of the material points up differences, we find in Chapter 2 (Perception and Motivation in R & D Management) that the conceptual schemes outlined there, and suggested by the authors as being useful for analyzing the data in the R & D cases to follow, have

been used in the past to analyze cases dealing with the behavior of people working in every imaginable organizational component. Faced with this apparent *non sequitur*, we should note that these particular ways of conceptualizing the data to be studied in this book have been deliberately selected from many other ways of thinking about behavioral date because they have been found especially useful when students of management have been discussing the problems of professional people.

Often the participants in such case discussions will, themselves, be people with professional degrees. They often find it helpful if they are able to recognize at the outset that *their own perceptive world*, and that of the other participants in the discussion, has a profound effect on their analysis of data. In this paper, therefore, we have spent a good portion of our time talking about the factors which influence perception and ways of spotting a particular perceptive "set," either in the classroom or in the cases themselves. Referring to another concept discussed in Chapter 2, the particular *incentives which motivate* professional people, while similar in kind to those which motivate nonprofessional people, need to be spelled out rather exactly (as we have done) in order to make the more generalized concept of motivation discussed in Chapter 2 operational for professional people.

Experience has demonstrated that El Paso Electronics, Cannon Laboratories, and Eastern Electronic Co. are good early cases which prepare those discussing them for the more difficult cases to follow. Each case appears to evolve around a specific problem, but further examination usually leads the discussion group to the realization that only the tip of the iceberg is showing.

Research Studies

We have grouped together all of those papers which we call "Research Studies" in Section II, and it will be found that particular studies appear specially relevant to one or more of the cases which follow in Section III. Moore and Renck's paper (Chapter 6) is of general interest, but applies particularly to several of the cases, including United Deisel (Chapter 18). It would be helpful to read and discuss Chapter 7, dealing with the value orientations of managers and scientists, in connection with the Industrial Chemical series of cases (Chapter 13). While differences in values influence behavior in many other cases in this book, these differences are quite apparent in Industrial Chemicals and need to be understood if the reader is to comprehend what is happening in this particular organization.

By way of contrast, Chapter 8 posits some discriminations which are not to be found in the particular form outlined by Barnes in any of the case material. In no instance, to be specific, are characters described in the cases as "professionals," "organizationals," or "socials." The be-

havior of many people in many of the cases, however, would lead the analyst equipped with these particular discriminations to label certain people accordingly. Having identified the role type, one may see in the clinical data of the cases some of the ways in which "organizationals" behave and react differently than "professionals."

In the same way, as the student observes the behavior of those in positions of leadership in case after case, it will help to become familar with the different styles of leadership defined by Baumgartel in Chapter 9. Baumgartel's research demonstrated that the consequences of different styles of leadership tend to be similar in almost any organization, and we see this truth repeatedly demonstrated in the cases.

Kaplan's research, reported in Chapter 10, identifies some of the factors which appear to influence creativity. It will not be difficult for those studying the cases to identify these factors as they crop up in the organizations studied and to speculate on the impact of them on creative efforts in the laboratory environment.

Of all the problems which influence the effectiveness of R & D organizations perhaps the most prevalent and most difficult to deal with arise in connection with trying to achieve cooperation between research laboratories, development groups, and production departments. In his paper (Chapter 11) Tom Burns reports the result of extensive research, which enabled him to identify some of the pervasive factors which make these relationships so difficult.

Particularly in these days of rapid technological obsolescence, problems of change in industrial laboratories are much with us. Sometimes the changes which must be made create complicating problems for individuals or groups within the organization. In Chapter 12, Clovis Shepherd reports on a research project where the focus of study was a laboratory in the process of change. As a result of (1) the specific changes that were made and (2) the way they were made in this laboratory, several conflicting factions developed. Shepherd found that some of the issues which resulted in this factionalism involved conflicting objectives, in-group loyalties, and personal relationships. In several of the cases to be studied the reader will be able to see many of these forces at work. We would particularly suggest study of Shepherd's paper in connection with the Atlas series of cases in Section V.

Issue Cases

In many ways, Section III is the focal point of this book. The 20 cases deal with most of the important issues which arise around the behavior of people in R & D organizations. Some of these cases have been taught many times in classrooms all over the world, and others are brand new and are in print here for the first time.

The Industrial Chemicals series, and particularly the (A) case, belong to

the former category. Under another name these cases form the body of data for a research report published in 1963.[1] The cases themselves have been reprinted in several other case books and have been discussed many times by groups of managers, groups of scientists, mixed groups, and students. The (A) case, dealing with the problems faced by Messrs. Shaver, Mace, Hyler, and Coker, has more than once formed the basis for an entire day's discussion.

The Industrial Chemical series is a particularly useful one since the data help the reader to "zero in" on many familar aspects of human behavior in research organizations. Each case in the series focuses on a different issue, but many of the characters and the overall atmosphere of the specific R & D organization remain constant. The concept of the "rose colored glasses," discussed in Chapter 2, can be applied in full measure in analyzing these cases. In fact, this particular series provides a most useful medium for practicing this conceptual scheme.

Haig Chemicals (A) is another well-known and much-discussed case. It introduces issues revolving around problems of organizational change and intergroup relationships. The behavior of the president, Mr. Haig, and that of Chief Engineer Ryan, has been of particular interest to groups who have discussed this case.

Scientific Research Incorporated (Chapter 15) represents a change of pace. Here we are asked to examine a series of problems involving interpersonal relationships between a young scientist and the technicians working with him. In analyzing this case it will be found helpful to try to determine the *self-concept* being expressed by the major characters (see Chapter 2).

Utility Power Company (Parts I–V), a so-called "sequential case," represents a pedagogical departure from other cases in the book. This case is presented in such a way that each succeeding part adds additional data to that appearing in previous parts. Generally, those who are to discuss the case are asked to study Part I before the discussion begins. The discussion leader then requests the participants to read Parts II–V at intervals throughout the discussion. It is often considered useful to test the ability of the students to predict (on the basis of fragmentary data appearing in early parts of the case) the consequences of behavior described or of decisions made.

The Bob Knowlton case (Chapter 17) deals with two often-faced problems, which in this case are related to one another. We see here the problem of the brilliant individual versus the group, and at the same time, confusion about the roles to be played by various individuals working with each other.

The United Deisel Corp. case deals with another kind of intergroup

[1] R. M. Hower and C. D. Orth, 3rd, *Managers & Scientists*, Division of Research, Harvard Business School, 1963.

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problem: the relationships between draftsmen and engineers. Among the research readings which may contribute to an understanding of the problems presented in this case we would suggest, particularly, "The Professional Employee in Industry" by Moore and Renck (Chapter 6).

Another frequently encountered problem is the subject of the Acme Aircraft Corporation case. Here we see individuals and groups trying to cooperate under pressures of time, and we have a chance to observe and try to understand the tensions and confusion that results from such pressures.

The Dallas Chemical Corporation can best be described as an "issue" case. The issue concerned is the so-called "dual hierarchy" approach to advancement in an R & D organization. There is little data in the case to help the student of management resolve this issue, but the issue itself is raised in terms which will be familiar to many who have dealt with this problem. The case may be discussed in conjunction with Chapter 30, Shepherd's paper on the "Dual Hierarchy in Research."

The issue encountered in the Fort Worth Pharmaceutical case is frequently observed by organizational behaviorists but is less readily recognized by most R & D people. We find one Wayne Malvern trying to fill the role of a "linker" between research people and an advanced development group. The student may discover that the research reported by Tom Burns in Chapter 11 is rather pertinent in light of the problems Malvern is facing at Fort Worth.

The problems of many research and development organizations, some would say, often stem from the methods used to recruit the professional staff. If recruiting decisions are poor ones, the after-effects may adversely influence a laboratory's work for some time to come. In the Houston Corporation, (A) and (B), we have an opportunity to appraise a particular recruiting methodology.

The long American Metals Company case series, which concludes Section III, may be seen as a bridge between the "issue" cases in this section of the book and the more complex case series in Section V. Around such familiar problems as the evaluation of research results we expect to find, and do in this series, numerous issues involving the relationships of people in the R & D organization. Among the readings which may contribute to understanding of these data are Kaplan's research on "Organizational Factors Affecting Creativity" (Chapter 10) and Howton's paper (Chapter 28).

Conceptual Readings

As we stated above, the conceptual readings which follow the series of cases just described represent some diversity of opinion because of the particulars of the experience of the various authors of the papers in Section IV, as well as differences in background which led them to

interpret research results somewhat differently. The differences will, perhaps, be perceptible to the student more in terms of the basic assumptions concerning human behavior in organizations held by the various writers than in the form of sharp disagreements between them on the same issue or issues.

Man as a Research Tool, by Philip Sprague, which leads off this section of the book, is perhaps the most controversial viewpoint to be presented herein. Unlike most of the other readings in Section IV, this one was written by a working manager, president of his company, who sets forth in this paper a point of view based on experience. Inevitably, his point of view stems from a different perceptual field that those of the other people who contributed papers to this section. In contrast, these latter were writing from a point of view which arose out of their own research in R & D organizations, or were using the point of view of other behavioral scientists. We do not mean to imply a value judgment in either direction here; we are simply underlining a basic difference in background which inevitably results in a different point of view (and a different writing style).

Orth's article (Chapter 25) describes some basic factors which lead to a creative climate for research (especially, but not only, "basic" research). He suggests that the behavior of many managers, and the way they see their job in R & D management, is less than ideal if they expect such a climate to evolve.

This and the following paper may be usefully assigned rather early in a course using the material in this book. Shepherd's "Nine Dilemmas in Industrial Research" also ranges rather widely and is helpful to the reader trying to define the issue or issues which appear to be crucial to the analysis of most of the cases.

In "The Role of the Research Manager (Chapter 27), Steele focuses sympathetically on the manager himself. His role is a difficult one, says Steele, because he "sits directly astride the problem of how to achieve a workable accommodation between scientists and businessmen." The reading may appear particularly revelant to the student pondering the leadership styles of Dr. Dartell, in Continental Aluminum, or Dr. Kantrowitz in the AVCO-Everett Research Laboratory series.

The chapter from Sayles' book (Chapter 29) is included herein because the authors agree with Sayles that "The choice between perceiving the creative individual as omnipotent on the one hand or a passive instrument of organization forces on the other, is too narrow." Sayles spells out this point of view in a refreshing style which we feel makes this reading both interesting and informative.

The case series in Section V are too complex to describe briefly. In each, many of the issues discussed in earlier cases reappear. The Atlas Corporation series presents for analysis such problems as the relative status of individuals and groups: role development and interpersonal,

intergroup, and leadership behavior, all within the context of an organization under the pressures of change.

Continental Alumnium and the AVCO-Everett Research Laboratory both deal, among other things, with leadership behavior. In Continental, a dedicated laboratory manager has a profound influence on the development of administrative and technical people in a basic research environment. In AVCO we see an interesting balance between strong personal leadership and the development of a participatory pattern of management.

We have deliberately made this tour through the book a rather sketchy one. It has been our experience that a collection of data such as this is subject to many interpretations and we do not pretend to have an exhaustive knowledge of all of them. Even if we did, we would prefer to leave to the reader a voyage of discovery, allowing him to see relationships which we have not seen at all, or have perceived only dimly.

It has often been said that in the management of human affairs there are no "right" answers or final solutions; only that some ways of proceeding are better than the alternatives. As the reader considers alternatives, in light of the data in the cases, and works toward well-reasoned decisions regarding the action he might take, we predict considerable thought for him, illuminating analytical exercise, and stimulating, insightful discussions with colleagues who share his interest in and concern about the problems presented herein.

Chapter 2

PERCEPTION AND MOTIVATION IN R & D MANAGEMENT

WHETHER we are reacting to the concrete reality of the world around us or to the abstractions represented by the clinical data in a case, our response (or behavior) is strongly influenced by what we perceive and how we perceive it. Assuming that we hope to become more skillful in our responses (or behavior), it would seem apparent that more knowledge or insight about the role played by perception in governing our own and others' behavior would be helpful.

This can become a complex subject. Some of the sophisticated conceptual ideas we might extract from the considerable body of research which has been done in the field of perception might easily obscure the operational utility of simpler concepts. We propose here to relate a way of thinking about the role of perception as an influence on behavior which has been demonstrated, in the classroom and elsewhere, to have considerable utility.

There are a number of ideas in this book: ideas about management, about administering R & D, about the behavior of scientists in industrial organizations. In some ways the fundamental ideas we deal with in this chapter are central to all the others. A case discussion can become a very rewarding experience if both the instructor and the participants are constantly aware of, and become skillful in responding to, their own perceptual field and the perceptual fields of the other participants in the discussion.

Similarly, a committee meeting, often maligned as a way of getting things done, can be highly productive if all or even several of the members of the committee can respond skillfully to each other in terms of insights about their own perceptive fields and those being implied by the behavior of others. Consider this example:

A classroom group is discussing a case involving the status of PH.D.'s in an industrial laboratory. Strong feelings are being expressed but little insight about the underlying causes of the problems in the case seems to be emerging from the discussion. One of the participants in the discussion suddenly recognizes that while all of the participants work in industrial laboratories, very few of them have advanced degrees. He asks the group if this fact may not

be influencing the discussion. The group explores the implications of his question and the discussion proceeds at a much more insightful level.

Some people appear to be more adept than others at making use of the kind of skill demonstrated by this participant in the case discussion. In this instance, he was able to perceive an underlying sentiment or feeling that was strongly influencing behavior, point to a possible reason for the existence of the sentiment, and thereby help the group recognize that the sentiment was making it difficult for them to have a useful discussion. Note that he did not say that the feelings about Ph.D.'s being expressed by nondegree holders were *wrong*. He simply suggested that they might be *present* and exerting a strong (and in this case dysfunctional, because they were not recognized) influence on the discussion.

As we have said, this person displayed an ability to perceive a feeling or sentiment. This is only one of many perceptual skills. Some people appear to exert this and other kinds of perceptual skill naturally. Others, however, in our experience, have been able to *learn* these skills when they discover what to look for in their own behavior and that of others. This does not mean that they practice amateur psychiatry; they simply become more sensitive to certain common phenomena in the world around them, and more adept at applying the results of this sensitivity to everyday problems. In order to do this they need a readily available and not too complicated model or concept to serve as a guideline or checkpoint. A model which seems to work for many people is the diagram in Figure 1.

In the business school classroom we sometimes refer to this diagram as "The Manager's Rose-Colored Glasses.*" The spectacles represent the screen which we all carry with us always and through which we see or perceive the world around us. Inevitably, each of us is strongly influenced by the particular pair of glasses which is uniquely his own. Each pair of lenses has certain elements built into it, and while there may be common elements in the lenses of person *A* and person *B*, they are seldom exactly alike.

If all of us knew in detail how the elements in our glasses influenced our image of the world, and if everyone else carried a tag on *his* glasses noting all the elements in their lenses, this world would be a simpler (and much duller) place. The purpose of much interesting human intercourse is a mutual attempt to learn more about what the other fellow sees in the world and why he sees it that way. Unfortunately, we all too often jump to unwarranted conclusions in this area: we see

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* The term is a joking one. The glasses we will describe serve to *distort* one's image of the world more often than they correct or enhance it or make it seem "rosy."

and hear what we *want* to see and hear, or what we are *conditioned* to see and hear; we make judgments based on our own values, for instance, and ignore the legitimacy of other and contrasting value systems which may make us uncomfortable but which inevitably exert a strong influence on the behavior of those who hold them.

Let us be clear about one important point before we discuss the six elements appearing in the lenses of the "rose-colored glasses" in more detail. When we suggest that a more sensitive and analytical orientation toward the preceptual fields of ourselves and others embodies a useful skill which can be of great help to the manager or administrator

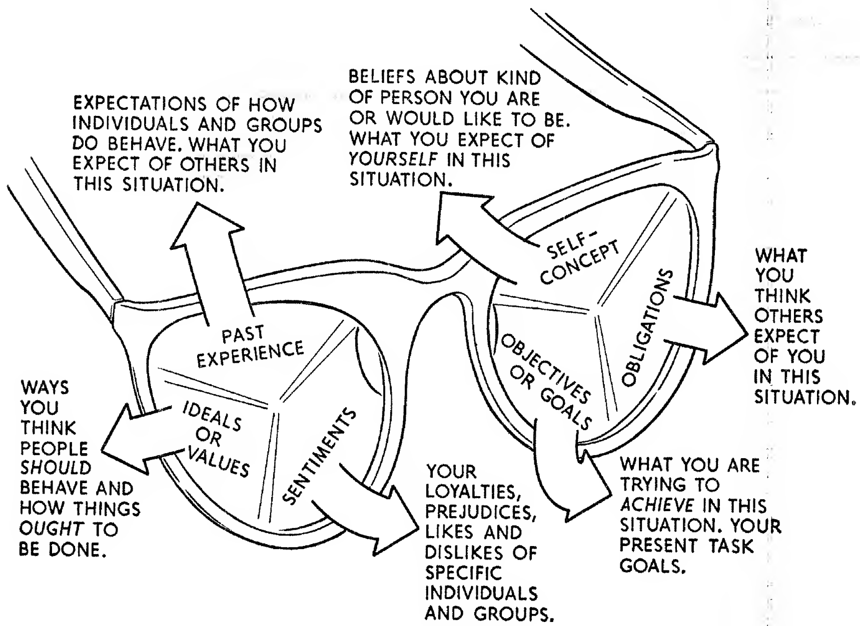

FIG. 1. Perception.

(particularly, but not exclusively), we are not suggesting that everyone should accept the behavior or ideas of everyone else. We are not likely to accept the behavior of a murderer—or even that of a colleague who disagrees with us. We *are* suggesting that it is often helpful to *understand,* or at least to make an effort to understand, behavior that appears to us to be, in one way or another, dysfunctional. (Sometimes this dysfunctional behavior is our own.)

The point to remember is this: *Behavior which is perceived as dysfunctional by other individuals or by an entire group is rarely perceived as dysfunctional by the individual displaying the behavior unless something in his perceptual field changes.* Most of us behave in ways that are internally consistent or congruent. If we can understand how be-

havior, which seems dysfunctional to us, makes sense to the individual behaving, and can state this understanding in nonjudgmental terms, we can often help another person see the dysfunctionality of his behavior without arousing a defensive reaction in him. Again, an example:

As a laboratory manager you have cause, you believe, to tell one of your section heads that he must learn to do a better job of "selling" the research of his group to top management. Putting yourself in his place, you recognize that he feels he must "behave like a scientist" if he is to maintain the respect of his group and that, as he sees it, "selling research" is not scientific behavior.

If you hear this feeling expressed, it might be wise to reflect it. "John, I know you feel a strong sense of loyalty to your group," would be one possibility. Or, if you feel it makes sense in light of the way he is talking, you could be more explicit: "John, you do not believe a scientist should be put in the position of having to sell research."

John, feeling free to explore the implications of his beliefs, may end up by deciding that he can maintain his scientific reputation while doing a better job of "selling" the research of his group. It is just possible, of course, that you, as laboratory manager, may end up by agreeing with John that he should not "sell research" and by exploring with him alternative methods of accomplishing your purpose. In this instance, you may have learned something about your *own* perceptual field, and this insight may help make you a more skillful manager.

Returning to the "rose-colored glasses," we need to explore the implications of the six variables which make up the elements of each individual's prescription.

1. PAST EXPERIENCE

The value of experience is relatively unquestioned. Under the best of circumstances we learn from our experience and the sum total of these learnings at any point in time represents our character, our skills, and our values. As a product of all of these, and other variables, our behavior is determined.

It is also true that some of us do not learn from our experience, or at least fail to learn the kinds of lessons which are helpful to ourselves or others. And then there are those, perhaps the majority of us, who learn some helpful things from our experience but also some other things which tend to make life difficult for us.

We are conditioned by our cultural heritage. Inevitably we learn from the world that we are taught, or learn what we can observe for ourselves. These "lessons" may be limited by lack of opportunity, by the peculiarities of our backgrounds, or by the particular characteristics of those who teach us. Some examples may serve to illustrate the points just made:

A white person brought up in Mississippi has probably had experiences with colored people which conditioned him to expect quite different behavior from negroes than that normally expected from the negroes known by Parisians or

Londoners. The culture of the southern part of the United States may very well condition a white man to believe, for instance, that "negroes are lazy." A Londoner, who has learned that some colored men graduate from Oxford and Cambridge and enjoy distinguished careers, may find the generalization "negroes are lazy" less than adequate.

A laboratory technician, who has observed many scientists working past midnight, dressing in a nonconforming manner, and interacting with each other in (to him) "peculiar" ways, may base the familiar "scientists are screwballs" stereotype on these observations. A scientist, on the other hand, who has experienced a need for concentration in his work, who has been trained to think and behave in nonconforming terms, and who enjoys the "in group" camaraderie of his peers would not understand, and would tend to resent the "screwball" appellation if it were verbalized in his presence by the technician.

We recall reading *The Silent Language* by Hall[1] and being struck by one of his many fascinating observations. Hall points out that people have varying degrees of tolerance for physical and emotional proximity to other people. Englishmen, as a nationality, for instance, prefer to "keep their distance," whereas Italians and other Latins embrace frequently and talk or argue volubly with their noses inches apart. We tested this observation one time by moving close to an Englishman while talking with him at a cocktail party and, by repeating this behavior, managed to back him up 30 feet in 15 minutes. (What did we learn from this experiment? If we tend to stereotype we might learn that "all Englishmen will back up." Only a punch in the nose helps some people to unlearn such lessons.)

Past experience and the expectations about behavior that result from our on-going experience can be not only valuable but, obviously, a very necessary facet of total learning and developing maturity. As the examples cited above suggest, however, there are dangers involved.

What do we *select* from the total situation in front of us and why? We cannot possibly "see" everything in the world around us. We need to categorize (that is a dog not a cat), systematize (dogs usually like meat but rarely eat hay), and often, in the interest of time or safety, stereotype (beware of dogs: they bite). In turn, each of these ways of learning from experience may work against us: we can overdo our categories, under-complicate our systems, and fail to examine our stereotypes in light of new evidence.

Operational Implications. Beware of the possibility that your past experience predisposes you to certain kinds of categorizations, particular ways of systematizing the categories you choose, and inevitable stereotypes. Become *more* aware of the particular categories, systems of categories and stereotypes that are a product of your own experience, as well as those exhibited in the behavior of other people. Try to develop a more sophistocated understanding of the impact of these influences on your

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[1] Edward F. Hall, *The Silent Language* (Garden City, N.J.: Doubleday & Co., 1959).

own behavior and that of others. Finally, try not to shut off your ability to continue to learn "useful" lessons from experience. (None of this is easy; some people spend a lifetime trying to accomplish these ends without progressing very far.)

2. IDEALS OR VALUES

As past experience may enable us to learn valuable lessons about what to expect from people or groups—or obscure our vision behind a haze of inaccurate categories, too-simple systems, and rigid stereotypes—so the ideals or values which sometimes result from our past experience can guide us along "sure" pathways—or impel us into one dead end after another).

It is difficult indeed to measure the impact of values on behavior. It is even more difficult to argue that the impact is not there. The world of science, and particularly the world of science in industry, offers us much evidence of this. We will explore some of this evidence elsewhere in this volume.[2]

Suffice it to say here that our "oughts" and our "should-be's" are often so much a part of our character or essential being that it is most difficult for us to question the functionality of these views. Change is always difficult in any sphere; a change in values or ideals is perhaps the hardest of all to make. Each of the Ten Commandments can be quite sensibly attacked as dysfunctional under certain circumstances. Yet most Christians believe deeply in the moral code they summarize and have, through history, fought and died to defend one or another of these normative "rules" of human behavior against efforts to change them.

The role played by religious, social, business, or scientific value systems in governing behavior has aroused considerable ambivalence among philosophers and psychiatrists alike. Some observers of modern times will assert that the Protestant ethic has led many men toward a less than optimum development of their potentialities ("All work and no play makes Jack a dull boy"). Others will argue that the lack of similar strong values has led young men and women of the 1960's toward a lack of direction or purpose, a confused, hopeless, unreal point of view toward themselves and their place in society. The fact that both points of view are supported by considerable evidence seems to us to underline the powerful, but hard-to-tie-down influences of values on behavior.

Operational Implications. It behooves one to try to understand the values underlying the behavior of oneself and others. Whether you then accept, or question, the values, you understand them and can assume that

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[2] See especially Chapter 7, Value Orientations of Managers and Scientists, by Renato Tagiuri.

they do not change easily. While defending your own ideals, appreciate the legitimacy of differences and try to work toward action which accommodates the several value systems which evolve.

3. SENTIMENTS

Feelings are facts and are, as well, an important element in our "rose-colored glasses." A feeling itself may not be a fact, but it is a fact that someone feels a particular way at a particular time in a particular situation. Feelings are often nonlogical, often totally dysfunctional ("I feel like cutting the boss' throat"), and often unexpressed or very difficult indeed to interpret, account for, or explain. But the loyalties, prejudices, likes or dislikes we all experience are primarily influences on our behavior and cannot be ignored or brushed under the rug just because they make us feel uncomfortable.

Past experience has taught many of us to stifle our own feelings, or at least to avoid expressing them except under favorable circumstances. Particularly strong feelings involving love, hate, insecurity, conflict, fear, or passion may be considered socially unacceptable. Scientists and other technical professionals are reputed to be particularly prone to feeling-avoidance, and yet we all know scientists who express their feelings both freely and well. We can see a number of examples of such feeling-expression in this book: note, for instance, Dr. Coker in the Industrial Chemical (A) case and Dr. Capa in the American Metals Company.

It is in this area that many managers become fearful of their own inadequacy. Particularly in their role as "the boss" they do not see how they can deal with the feelings of subordinates, even if expressed, and cannot see the function of encouraging the expression of feelings, particularly negative ones. Real skill in handling feelings does not come easily to most people. Even a trained psychiatrist or skilled psychologist finds himself wrestling with difficult problems involving his own feelings toward his patients. In organizational life the manager can be excused his feelings of inadequacy, but we have found that if he can overcome this belief in his own inadequacy he can probably, with relatively little training, acquire at least some skills in dealing with feelings, in sensing the impact of feelings on what people in his organization believe, and, consequently, understanding better how they behave. Perhaps an example involving the writer, who is supposed to have some skills in this area, will serve to illustrate several of these points:

In charge of an office where both formal and *ad hoc* meetings of the staff are frequent, we found ourselves in a situation which began when one staff member came in to discuss her future role in the office. The discussion began to involve other staff members, who either heard what was going on and came into our office, or were invited in by us.

The next morning a senior staff member, who had not been in on the im-

promptu meeting, angrily queried "How come I wasn't invited to the staff meeting?" The writer's reaction was immediate. Such statements as "It wasn't a staff meeting" and "It never occurred to us to count you in because the problem didn't concern you" were his almost automatic response to the anger and other feelings being expressed. In effect our reaction was a defensive one, and certainly not very skillful. On the other hand, belated recognition of our defensive reaction enabled us later to explore and accept (and learn from) the feelings underlying the staff member's angry reaction.

Operational Implications. Try to be sensitive on a day-to-day basis to a fact of life we all appreciate: that feelings guiding behavior are often unexpressed. People are often willing to share their feelings if the climate is nonjudgmental and nonpunishing, and if the listener's own feelings (uncomfortable ones usually) do not tend to block feeling-expression from others. Feeling-expression *is* sometimes dysfunctional: people need their defenses. But the need to express is often more urgent than the need to defend. It is seldom a mistake to try to perceive more accurately the feelings of others and to try to create a climate in the organization where feelings can be dealt with in a mutually supportive manner.[3]

4. SELF-CONCEPT

Some of the most pervasive beliefs we hold are those we hold about ourselves. Our beliefs about the kind of person we are (or would like to be) can profoundly influence the way we see the rest of the world and, consequently, our relationships with other people. Some examples:

The chemist who sees himself as a CHEMIST and who therefore perceives "basic research" as the only career opportunity which dovetails with his self-image.[4]

The engineer who believes he is more intelligent than his colleagues and therefore finds it difficult to accept help from them.

The laboratory manager who sees himself as a very likable person, and who is shattered and unable to function when he finds one of his subordinates "cordially" dislikes him.

The participant in a case discussion who believes he is a poor speaker, and therefore never talks in a classroom.

Tapping into our own self-concept is difficult enough; understanding the self-images of other people, in any real way, is probably a job for a professional. There are, however, many useful clues that crop up in the normal course of day-to-day events and, if we can learn to recognize them

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[3] For an excellent exposition of this point, see Roger Harrison, "Defenses and the Need to Know," *Human Relations Training News*, National Training Laboratories, Washington, D.C., Vol. 6, No. 4 (Winter, 1962–63).

[4] One of the best treatments of this subject in the literature can be found in Bernice T. Eiduson, *Scientists: Their Psychological World*, Basic Books Publishing Co., 1962. (See esp. Chapter 5)

and sense their importance, we can often utilize such insights to our advantage—and, hopefully, to the mutual advantage of the other person involved and the organization as well.

An individual's self-image sometimes changes (when it changes) as a result of a discrepancy between the way a person sees himself and the way his culture sees him. Even then it only changes when this discrepancy is somehow communicated to, and internalized by, the individual in question. For instance:

> Dr. Bonney, a chemist, transfers from the research laboratory of his company to the development laboratory. In the research laboratory, Dr. Bonney had not been highly regarded and came to see himself as a second-rate chemist. Seeing himself this way, he became despondent, the quality of his work was poor, he tended to react defensively to the slightest criticism. In the development lab, however, his new colleagues reacted to his work in a highly complimentary manner. He was praised several times for his contributions by laboratory management. He began to see himself in a different light, took pride in his work, and, as a result of this new positive outlook, developed enough self-confidence to trade critical reactions with his colleagues and with management without displaying a need to defend himself.

What changed? There were really three changes here: (1) in the environment, (2) in Dr. Bonney's perception of himself, and (3) in his behavior.

In this volume, Dr. Coker in Industrial Chemicals (A) sees himself and his role as a supervisor somewhat differently as the result of a long conversation with Dick Payton. Here the environment did not change, but the individual's evaluation (perception) of it did change, and this, over a period of time, led to substantial changes in his self-image and behavior.

Operational Implications. Listen for statements which express or imply a facet of an individual's self-image which may be influencing his behavior. Depending upon the circumstances, you may or may not decide to question or try to state the relationship between the self-concept and behavior you have perceived. Unless your personal relationship with another person can stand the strain of possible misevaluation, and unless you are confident you can explore such matters without expressing or implying value judgments, it is probably wise not to probe too deeply. Simple understanding of the role of self-concept in determining behavior may lead to many useful insights, and this alone may enable you to behave more skillfully in your relationships with others.

5. OBLIGATIONS

Nearly everyone is influenced to some extent by what he believes others expect of him. The role we play is sometimes ill-defined but more often is well enough delineated so that we can perceive fairly clearly the expectations others have of us. Doctors or ministers, for instance, occupy

well-defined positions in our society, and their behavior is inevitably influenced by their understanding of what society expects their behavior to be (the occasional maverick notwithstanding).

Similarly, scientists and managers carry on their shoulders a number of culturally induced responsibilities and consequent obligations. Note that individuals differ widely in their perceptions of the obligations their role or status imposes upon them. Their behavior is likely to be guided by what they *think* their obligations are, not by some definition of responsibility imposed upon them by others which does not dovetail with their self-image.

The well-defined "man-in-the-middle" position, which so often engulfs hapless managers in industry, is definable as a conflict-in-perceived-obligations situation. Referring once again to the Industrial Chemicals (A) case, we see Dr. Hyler in such a position. On the one hand, he feels he has to defend the values of the scientists working in his research department. On the other hand, he feels he must represent the conflicting values of company management as these are articulated by his boss, Dr. Mace. Inevitably, his perception of his obligations places him in a difficult role, one which he obviously does not understand or know how to handle.

Some individuals find it very uncomfortable to be in situations where their obligations are ill-defined or ambiguous. In their struggle for stability or security they will invent obligations where none really exist. Other personalities have a relatively high tolerance for ambiguity; such individuals, for instance, are rarely over-concerned about their status in an organization and appear to have little need for a well-defined job description. Since they perceive themselves as "free agents" with a wide range of obligations, which in turn shift and change according to the particulars of the situation, such people can exhibit a considerable range of behavioral characteristics. In this connection it is interesting to note a definition of interpersonal competence promulgated by our colleagues David Moment and Abraham Zaleznik:[5]

> Interpersonal competence can be viewed as the capacity of an individual: (1) to work within a broad range of the spectrum of behavior; (2) with a minimum strain on the person's defensive system; and (3) with the optimal use of energy available to the person.

Operational Implications. Since most individuals tend to perceive their obligations as a factor of their reference group affiliation[6] or of their status in an organization, and since the range of obligations tolerable to different people varies widely, the manager needs to be sensitive to the

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[5] See *Role Development and Interpersonal Competence* (Cambridge, Mass.: Division of Research, Harvard Business School, 1963), p. 158.

[6] See Chapter 8, entitled "Organizational Systems and Engineering Groups," by Louis B. Barnes.

resulting differences (in himself and others) and the influence of such differences on behavior and performance.

6. OBJECTIVES AND GOALS

The fact that what you are trying to achieve, either in a particular moment or over an extended period of time, influences your behavior, is obvious. Almost anyone who has a well-defined goal toward which he is moving with dispatch will tend to see in the world around him only those things which will, in some way or another, influence his progress along the pathway toward his objective.

If we are in the fortunate position of having a well-defined goal or goals and at the same time recognize the fact that our pursuit of this goal tends to make us impatient with factors which are tangential to our objective, we can usually avoid trampling over other people in our anxiety to reach the objective.

Most of us are not in such a fortunate position. Our goals are not so well-defined or we are pursuing a number of goals at the same time. Some of these goals may not be very clear to us and we may be behaving in ways which we ourselves cannot understand because our objectives are fuzzy.

There are other occasions where we may know our own objectives full well but, for one reason or another, choose not to let others in on the secret. In the latter instance we have what may be called "concealed agendas." We are motivated by objectives which are not known to those around us. Our behavior, therefore, may be difficult for those who do not know our objectives to understand. It is one thing to behave mysteriously when this is a part of your strategy; it is another to behave mysteriously because you do not realize that the impact of your mysterious behavior is making it difficult for those around you to relate to you in any useful way. A peripheral effect of the concealed agenda is that it often causes people to put their own interpretation on your behavior. Their interpretation may vary quite considerably from your own and this might make it considerably more difficult for you to attain your goal.

Behavior associated with goal achievement may tend to polarize in this way: On the one hand we see an individual who might be described as "goal-less," lacking any goals which really motivate him. Such a person could be described as a "drifter" or a "dilettante." At the other pole we find the epitome of the type discussed briefly above, the sometimes compulsively uni-focused individual. Such a person sometimes turns out to be very successful but often displays highly neurotic behavior. The problem of the uni-focused person is that in order to maintain his focus he must shut out the rest of the world, and therefore he inhabits an "unreal world," which is a simple definition of the term "neurotic."

Goals and objectives are not the only spurs to individual motivation.

People may, in fact, be strongly motivated by every one of the elements in the lenses of our "rose-colored glasses." It is entirely possible, for instance, that a deeply held value or a strongly felt sentiment will override even long-term objectives to the point where the resulting behavior will be in obvious conflict with the goals sought.

Operational Implications. Well-defined objectives *are* important motivating influences. As we have noted, an objective may be well-defined to one individual but concealed from others. In organizational life, therefore, the ferreting out or explicit statement of goals and objectives becomes necessary. For the manager this may pose a real challenge to his skill since he often perceives himself to be "in charge of" the setting of objectives. It is difficult for him to believe that important goals of other individuals in his organization might be quite different from those he believes are best. Once again, if he is to "zero in" on essential information which will help him to do a better job, he must cultivate the skill of listening for the implied rather than only the expressed, and the concealed as well as only what is easily perceptible. He must either take off his glasses or recognize the ways his perception is distorted when he has them on.

Before we progress to another subject, it may be well at this time to make some summary statements. We have discussed a number of factors which influence the way all of us perceive the world and which, therefore, influence our behavior. The important point to remember about each of these factors (or elements of our "rose-colored glasses") is simply that *they are there* and do exert influence on behavior—our own and that displayed by other people. By working on the development of *perceptual skills* (which means very simply that we recognize that we do have a pair of glasses on, and so does the other guy) we can often improve our ability to behave more functionally.

Incidentally, most of the examples we have given of the ways various elements of our "rose-colored glasses" influence perception and, consequently, behavior, have been drawn from organizational or interpersonal situations. The point should be made that a group discussing a case in the classroom is in some ways an organization. Certainly the same kind of perceptual and other interpersonal skills we have outlined above are as applicable in the classroom, or in any situation which involves a group discussion, as they are in the organizational context. The case method instructor who finds in his class even a few individuals who are making use of the skills we have described, or who, in the broader sense, recognizes that they are wearing a "pair of glasses" and behaves in terms of this recognition, is indeed a fortunate teacher. A case discussion which is at least partly influenced by people using these skills veritably "sings": insights pile one on top of another; "knowledge about" becomes "knowledge of acquaintance"; in short, as in organizational life, the socially skillful individual can exert a fantastic impact on even a large group.

MOTIVATION OF SCIENTISTS IN INDUSTRY

We have briefly discussed motivation in relation to the perceptive field of individuals as screened through their "rose-colored glasses." Let us turn around for a moment and talk more explicitly and from another frame of reference about the motivation of technical people in industry. Please refer to Figure 2, and note that every person has certain needs. In organizational life particularly, we may have opportunities to satisfy these needs, and as a result certain rewards may accrue to us. In our diagram we have described the essential needs which tend to motivate any individual in such a way that the categories we have used may be seen as a "hierarchy of needs."[7]

NEEDS	SAFETY	BELONGING OR MEMBERSHIP	STATUS-PRESTIGE, SELF-ESTEEM	SELF ACTUALIZATION
OPPORTUNITIES	OPPORTUNITY TO WORK AT A JOB WITH ADEQUATE PAY AND FACILITIES.	OPPORTUNITY TO INTERACT ON A MEANINGFUL BASIS WITH PROFESSIONAL COLLEAGUES WHO ARE INTERESTED IN HIS FIELD.	OPPORTUNITY TO DO REALLY INTERESTING WORK AT FRONTIERS OF ENGINEERING OR SCIENCE.	OPPORTUNITY TO WORK AT FULL CREATIVE POTENTIAL ON PROJECTS OF HIS OWN CHOOSING.
REWARDS	SECURITY	SOCIAL SUPPORT	RECOGNITION	INDEPENDENCE

FIG. 2. Incentives for scientists and engineers.

For instance, the need for *safety*, that is, for a roof over our heads, a living wage, or protection from our enemies, may be regarded as a fundamental need of mankind. Until this need is satisfied, no other needs will tend to motivate an individual: the need for safety is paramount. Once safety needs are satisfied, however, the need *to belong* or to become a *member* of a group or society becomes the primary motivating force. In turn, when one feels secure as a member he begins to think of *status, prestige,* and *self-esteem.* Few people who have not satisfied their needs for membership in at least one group find themselves able to press on to a position of leadership.

Maslow points out that there is one more level on the "totem pole" of

[7] The hierarchy of needs concept was first postulated by A. H. Maslow. His original theory is presented in detail in *Motivation and Personality*, Harper & Bros., 1954, and an operational expansion of it may be found in Louis B. Barnes' *Organizational Systems and Engineering Groups*, Division of Research, Harvard Business School, 1960.

needs; he calls this *self-actualization*. True self-actualization, in the sense Maslow describes, is rarely found, since he means by this term the discharge of the full creative potential of the individual. The fact that self-actualization is a motivating force, however, is clear: we can even see around us some individuals who have in effect jumped from the safety need level, bypassing needs for belonging or status, in the hope that they may become "their true selves." In organizational life particularly, this is sometimes accomplished only with considerable sacrifice.

It should be noted parenthetically, as our last paragraph suggests, that Maslow's "hierarchy" is in some ways misleading. Many people, for instance, are motivated by needs to belong and to achieve status at one and the same time. Again, referring to organizational life, this often throws them into a difficult conflict situation. As many researchers, novelists, and movie producers have pointed out, it is not easy to belong and achieve status at the same time: the "achiever" in any society may accumulate a number of enemies, or lose many friends, along the way.

Returning now to the specific *opportunities* for scientists and engineers in industry to achieve their needs along the hierarchy or spectrum we have described, we can note that the safety need is satisfied relatively easily. The opportunity to work at a job with adequate pay and facilities is usually open to professionally trained people.

In the typical industrial laboratory, however, it may not be quite so easy to satisfy a specific need for belonging and membership. Whereas a nonprofessional (Barnes, in the study referred to previously, calls them "organizationals" or "socials") may satisfy his need to belong by becoming a member of almost any group in the organization where he finds compatible people or people who share his values, the scientist or engineer requires membership in a very special group, one which gives him an opportunity to interact on a meaningful basis with professional colleagues who are interested in his field.

Given the values of science (or engineering) which have been ingrained into a man professionally trained in these areas, it is likewise true that his need for status, for prestige, or for self-esteem cannot be satisfied in as broad a sense as may be true for the manager or the worker in fields where the value system is less implicit. The scientist who has internalized the values of science can only achieve need satisfaction at the status-prestige level if he is given the opportunity to do really interesting work at the frontiers of science or engineering.

We have said that the need for self-actualization is seldom satisfied in modern industrial life. Those men, those few men we have known who have come close to satisfying this need, have been those who have, we often think, stumbled into situations where their needs and values and skills have been fully matched by the organization or industries where they have worked. They have found an opportunity to work at full creative potential on projects, in jobs, or toward goals which are of

their own choosing and which are, in turn, fully congruent with the needed projects, the desired goals, or the necessary jobs of the organization.

The product of the motivating needs and the opportunity to achieve these needs is a system of *rewards*. The professional technical person who has satisfied his safety needs achieves *security*. If he has satisfied his needs for belonging or membership he has achieved the *social support* which is so necessary to many people. Whether or not he achieved social support, if he has satisfied his needs for status, prestige, or self-esteem he has probably received *recognition* in his professional field. Few people in this world appear to achieve complete *independence* in the true and complete sense of that word, and some who do may spend their latter years reflecting on the painful, to them, consequences of their achievement.

The manager of research and development activities, who seeks to develop an organization where creativity may flourish, needs to understand and deeply appreciate the needs, opportunities, and rewards which motivate men with professional training. The creation of an organizational climate where people can, in fact, work at their full potential is a worthwhile goal—possibly an unobtainable goal but surely an objective toward which any manager may direct his full effort, secure in the knowledge that any progress he may make will be rewarding to himself, to others, and to the organization as well.

Chapter 3

EL PASO ELECTRONICS CORPORATION*

IN THE spring of 1959, Dr. Kane,[1] the manager of the research services laboratory at El Paso Electronics Corporation, was deep in thought when Bob Handy, a casewriter from the Harvard Business School, entered his office. The research services laboratory had been formed only three months ago, merging all the various service functions within El Paso's research center which required highly talented and educated personnel. Such services as electron microscopy, mathematical analysis, and computing, to name a few, had been growing increasingly important to El Paso's laboratories as work being done in basic research became more complex.

Looking up from a letter he was studying, Dr. Kane said: "I'm glad to see you received my message and could make it to this meeting. We have a very interesting problem which I must admit has me baffled. This letter is the first symptom of the overall problem and I am anticipating several similar letters in the near future. It is from one of the men in the mathematical group, a topnotch analyst, who is resigning and going with another company. The letter is very interesting as he seems to have thought this step through quite thoroughly, and it presents his reasons for leaving in detail. Here, take a look at it and tell me what you think."

BOB HANDY: Perhaps before I read the letter you could fill me in on the details and tell me something about the development of your organization.

DR. KANE: Certainly. As you probably know we are a collection of all the service groups that do professional work. This includes mathematical analysis and computing; material analysis which does X-ray spectrography, electron microscopy, chemical wet analysis; and a technology group which builds prototype electronic devices and provides researchers with quantities of new materials to their specifications. In each of these groups degree-holding professionals are a substantial percentage of our total staff.

BOB HANDY: How did this collection of divergent interests assemble under your supervision? What is their historical background?

DR. KANE: At one time or another most of the services in this

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[1] All names disguised.

laboratory were located in the labs as a part of their research efforts. Originally either a researcher had become interested personally in one of these fields or the work he was doing required one of these special services. There being no one to do this work for him he obtained the equipment required and developed methods of his own. Usually the man or men involved in the service had performed this work only as a side interest to a main research activity. However once it is established that such a man has knowledge about a field or has done previous work in a field, all other researchers turn to him to get their specialized work done. He has the experience; usually these requests start coming from the personnel in the man's own lab. The lab director approves of using this man as a specialist in a particular service area; soon the researcher has a service group established around him and is spending most of his time in service work. The next step has usually been a removal of this service section from the laboratory either because administration of such activities absorbs too much of research management's time or other labs start requesting these services, and to be fair to all a service group should not be attached to any of its users. Situations such as this were the typical way these service groups were formed, and it was only three months ago that they were collected into one group with the status of a laboratory. Does this give you an adequate historical background?

Bob Handy: I think so. Let me take a look at the letter which you mentioned.

Dr. Kane: By the way this fellow informed me that he was leaving before I received his letter. He said that he wanted to transmit his feelings in writing both to me and higher management.

The letter read:

Dear Dr. Kane:

As I have told you in our conversations on the eighth and ninth of June, I am resigning my position as senior analyst in the research services laboratory. The purpose of this letter is to document and explain my reasons for resigning. As you know, prior to joining the service group, I was a member of the technical staff in the basic physics laboratory. When that laboratory purchased some computing equipment, I was assigned the responsibility of aiding the laboratory members in the use of the equipment while maintaining my own research program. Eventually the computing and analytical work took such a large amount of time that I dropped my own research. Subsequently our group was removed from the basic physics laboratory and joined research services.

With this as background I would like to detail my reasons for leaving:

1. My training, ability and interests lie in the field of research, not service. I do not see any possibility of my returning to research and very little hope of doing much real research in my present position; the demands for service are too great.

2. The status of my position both within and without the labs is much lower now than when I was doing research. I have even heard one laboratory director state, "The service groups are of secondary importance; the labs can get lower-caliber people to do this work, and whenever a problem arises between

them and the researchers, it should be resolved in favor of the creative people." This has become especially noticeable in the past year with the increased emphasis on basic work and "real creativity."

3. I am losing my position in my professional field, for I am not publishing. This is because I am not doing anything worthy of publishing. Acknowledgements are nice, but they are not articles written.

4. I find that there is a limit placed upon my salary level, for I am not as valuable as I was as a researcher. This position also virtually excludes me from winning any of the yearly Merit Awards or the higher professional titles.

In essence my problem is a simple one; I have been removed from the lab's main line of activity, research. This shunting of my talents from the main line may be more valuable to the company in the total picture, however it has blocked enough of the important aspects in my development that I am unhappy. The only way I can remove these blocks is by my moving back to research and I have found this easiest to do by moving to another company.

Thank you for allowing me to state my case.

Very truly yours,
DR. F. K. ROI

BOB HANDY: Well he has certainly stated a strong case. What do you think about the arguments he presents?

DR. KANE: Unfortunately most of the generalizations he makes are true. Of course there are many people who are happy in service work, or who reached their limit in research and found this to be another avenue of growth. However this letter, in my opinion, only represents the beginning of what may be a serious personnel problem. The sad thing is that we have recognized these complaints and tried to counteract them with various solutions but haven't yet succeeded. Let me tell you what we tried to do and where I now believe we went wrong.

We felt that the major complaint which we were hearing was that the men were not doing research; so we initiated in most service groups a program of research with the professional people setting minor portions of their time aside for research on the services they are performing. For example, the math group was doing services research, like writing new computer routines, working out some new analytical techniques, and keeping up with new activity in applied math and operations research. The comments we received on work like this were certainly interesting. They ranged from "Is this an appeasement project?" or "Which has priority on my time, research or service?" to "I can't do research in patches of time fitted among my service work." If I could pinpoint any one thing wrong with what we did I would say that we didn't convince them that we *wanted* them to do research. They thought we were doing this as a consolation to them.

We tried other things as well. By consolidating these functions under a research services lab we put them on a par with the other labs so they would not be treated as stepchildren when it came to budgeting funds, personnel, or space. We hoped to improve their status within the organization by granting them formal management recognition. Here again we

have run into problems; separating the groups from the labs in which they originated has drawn the lines of distinction sharper. At the same time you probably recognize that scientific status in this organization is not determined by management as much as it is based upon the type of work you do, the papers you publish, and in general the contribution you make to science. In our hierarchy, service ranks lower than research, and management cannot do much about this without changing the jobs.

To answer the money problem is particularly difficult because we are so secretive about individual's salaries; we can't show the men the records and let them see that the salaries are similar. The thing that really hurts us in this area is that the really topnotch men who get the very high salaries and the awards and the titles are all in research. Service personnel hear about these people and blame the difference in remuneration on the laboratory they are in rather than on differences in abilities. The final problem we have attacked is the inability to publish. We have talked with the various section heads within the labs to encourage their men to coauthor articles with service personnel when substantial service work was done on a project. Some groups are very good about this while others don't care a bit. This has been a very spotty solution depending upon which lab group the service is for. Let's go to lunch. I promised Dr. Samuelson, the director of electronics devices laboratory, that we would have lunch with him.

During the luncheon with Dr. Samuelson, the topic of keeping good personnel in the service functions came up. Dr. Kane repeated the principles he had mentioned earlier but Dr. Samuelson seemed to take violent exception to them.

DR. SAMUELSON: I think that you are working under a false impression when you assume that to get the abilities and talents required for our service work we must use researchers and give them research to keep them happy. I don't think that you have considered whether or not service-oriented personnel exist with the talents we need. This is not your fault but is primarily due to our system of developing services within the research labs originally. Some of the problems you listed will still be important, but if you can remove the research orientation and motivation you have taken a big step. The easiest way to do this is to hire persons with these qualities from outside; but if this is impossible then we should consider breeding our own group of specialists from men with service orientation.

These two steps should be tried before we accept the fact that we need disgruntled researchers in these jobs, and to keep them we must throw them the sop of some research to do. If we have to do research on some of these service areas this is one thing, but doing it just to keep the personnel happy is quite another. I guess I sound pretty harsh and I don't mean to be, but we have had trouble getting our service requests filled and after all,

this is your primary job, since this research-on-services program was launched.

Back in his office after lunch Dr. Kane said: "You can see what type of problem I'm up against. I think that this is my most critical area, easing the professional personnel problems both immediate and the long range. I have to map out both a policy and plan of action, sell it to top laboratory management and hope that no more letters like this come across my desk."

Chapter 4

CANNON LABORATORIES, INC.*

A FEW HOURS before he was scheduled to leave town on an extended business trip, Dr. Eric Paisley, director of research for Cannon Laboratories,[1] found himself facing a serious personnel problem. The two scientists he had hired to establish a new antibiotic laboratory were both threatening to leave the company. Each man cited his inability to get along with the other as his reason for wanting to resign.

Dr. Paisley had been aware for some time that there was a considerable amount of conflict involved in the relationship between the two men but had hoped that he had managed to relieve the situation by talking to each of the men about it. The threatened resignations surprised him. He now realized that he would have to take some sort of action quickly or be faced with the collapse of the new and expensive antibiotic activity. He found himself at a loss, however, as he tried to decide what more he could do now about the situation.

Cannon Laboratories manufactured a wide variety of drug products in the "ethical" category. The research department under Dr. Paisley had been responsible for developing a number of these products, and a decision had been made to expand his department by adding to it a new division which would specialize in antibiotic research. Since no one then on the research staff was a specialist in this field, top management had decided to employ an outstanding microbiologist to head up the new work. After a number of scientists had been considered, the position was offered to Dr. Paul Booker. Dr. Booker accepted the proposal made to him and became active shortly after he joined the staff in drawing up plans and specifications for a new building which was to house the new division. Construction of this building began about the time when Dr. Booker started looking for his staff. He had already interviewed several scientists and had more or less made a definite commitment to one of them, Dr. James Waggoner, when he suffered a heart attack. Being advised by his doctor to retire from all business activity, he regretfully informed Dr. Paisley that he would have to resign his position.

At the time of Dr. Booker's resignation, the new building was in the

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[1] All names and certain other factual data have been disguised to conceal identities.

middle of construction, certain important items of equipment had been ordered and, as mentioned, at least one scientist was awaiting definite word regarding his employment. Although Dr. Paisley had had no experience in microbiology, he now found it necessary to take over active supervision of the construction and staffing of the laboratory.

It soon became apparent that no microbiologist of Dr. Booker's scientific stature was available to head up the new laboratory. Management decided to start by staffing the antibiotic division with younger men who would report directly to Dr. Paisley. It was hoped that eventually a suitable man would be found to take over the leadership of the group or that one of the younger men would prove to be capable of filling the position.

Although it was planned that the senior staff investigators who would eventually work in the new laboratory would all report on an equal basis, scientifically speaking, to Dr. Paisley, it seemed obvious that one of them would have to assume some administrative duties. The new laboratory was located at some distance from the rest of the research activities and from the office of the director of research. For this reason, and also because he himself possessed no training or experience in microbiology, Dr. Paisley believed that one of the scientists actually working in the new quarters should be made responsible for assignment of space, maintenance of the building, routine personnel problems, and the many other details which would inevitably arise in connection with the day-to-day operation of the laboratory. Concurring in this view, management decided that one of the three senior investigators to be employed should be assigned the additional responsibility of taking charge of the routine operations of the laboratory. He was not, however, to be given any special title.

Dr. Paisley resumed negotiations with Dr. James Waggoner, whom Dr. Booker had been on the point of hiring. Waggoner was a young investigator on the faculty of a leading university. Upon completing his doctoral work, he had worked for two years in a commercial laboratory very similar to the new division being established by Cannon Laboratories. Partly because of this experience, Dr. Paisley recommended that Waggoner be hired and given administrative responsibility for the new laboratory research activity. Having obtained management approval, Dr. Paisley wrote to Dr. Waggoner as follows:

DEAR DR. WAGGONER:

We talked about so many different things during your visit here that it is possible some of our ideas may have become confused or misunderstood. For that reason, I would like to present in letter form some of my own opinions on the matters we discussed as well as some of the more-or-less definite comments I made to you orally.

The general research policy so far established will be carried on with the new group as nearly as possible considering the different type problems involved. Suggestions for projects will come from various sources, but the individuals doing the work shall, of course, be the final authority on whether a

project can or cannot be carried out, and their opinion shall carry weight in consideration of projects. I, personally, do not feel in a position to direct specific biologic studies, so if you come here, you will be expected to work with freedom and independence. Establishing close association, mutual assistance, and exchange of ideas among the various research groups shall be my major effort in so far as the new laboratories and its personnel are concerned.

You appeared concerned that plans for initial operation of the new unit were to be made with only two senior staff men. I am certain that this is only a temporary consideration. In regard to this particular point, I shall repeat what I told you. Additional personnel must be acceptable to the senior staff man already employed. The administrative plan is for the senior staff men in the new laboratories to be responsible directly to me.

The matter of assistants and employees, other than senior staff men, as outlined by you is satisfactory, and I am sure that competent employees in those categories will be provided. Again, the final decision on such employees will be made by the senior staff man.

Finally, I would like to repeat the salary arrangement. Starting salary is to be \$8,500 with a promised increase to \$9,000 in six months, and subsequent salary considerations to be made annually. In regard to salary, I also want to reiterate that specific ceilings do not exist on any job. I feel sure I can arrange aid for moving expenses also.

Again, I enjoyed meeting with you and personally hope that you will decide to join our group. I think you will find the arrangement pleasant and productive. If there are any further questions or matters you would like to know from me, please do not hesitate to write.

Sincerely,
ERIC PAISLEY, PH.D., M.D.
Research Director

After further correspondence relating to salary and time for starting, Dr. Waggoner accepted the position and joined the company.

By the time Dr. Waggoner appeared, the new laboratory building was nearly completed. His first assignments were to order equipment for the laboratory and to help Dr. Paisley select another senior investigator. The two men interviewed a number of candidates during the next two months and finally selected one of these men, Dr. C. C. Holmes, as their first choice. In addition to having completed his PH.D. requirements with distinction, the prospective employee had had two years of postdoctoral research work at a university. Dr. Holmes accepted the position offered to him and joined the staff about six months after Dr. Waggoner.

Dr. Paisley told Dr. Holmes that he would be able to work independently on projects of his own; he also expressed the hope that Holmes would also work on some projects in collaboration with Dr. Waggoner. Dr. Paisley told the case writer later that he wanted each of the men to do some research with complete independence so that he might have a better basis for evaluating their performance.

As one part of the introductory conversations with Holmes, Dr. Paisley made a point of informing Holmes that, while he would not be responsible directly to Dr. Waggoner for his research efforts, Dr. Waggoner would be in charge of the routine activities of the laboratory, including super-

vision of the maintenance and technical help, purchasing of supplies, and dealing directly with the business department for the company on various expenditures for the laboratory.

Dr. Waggoner and Dr. Holmes proceeded to employ assistants, and begin research projects. Although each had his own office and laboratory facilities, certain laboratories were shared jointly. Informal meetings between Dr. Paisley and the two senior scientists took place "no more often than about once a week for an hour or so," as Dr. Paisley said, describing the situation later. Inevitably Dr. Paisley saw Dr. Waggoner and communicated with him, both by telephone and in writing, more frequently than he did in his relationship with Dr. Holmes. This frequency resulted from the administrative responsibilities which had been given to Dr. Waggoner.

After Holmes had been with the company for about four months, Dr. Paisley became aware that everyone, including people outside the company, was referring to Dr. Waggoner as director of the antibiotic laboratory, although no such title had ever been given him. Paisley heard indirectly that Dr. Holmes resented this practice, and he was inclined to be sympathetic with Holmes, since it had never been intended that Holmes would be subordinate to Waggoner. As diplomatically as possible, therefore, Dr. Paisley attempted to correct the erroneous impression which had become widespread within the staff group by speaking to people individually and explaining the relationship of the two men.

Two months later, however, Dr. Holmes complained very bitterly to Dr. Paisley's assistant director of research about the treatment he was getting from Dr. Waggoner. When he was informed of this conversation, Dr. Paisley asked Dr. Holmes to come in and talk with him about the situation. From what Dr. Holmes told his superior during the conversation which ensued, it was evident that he felt slighted by Waggoner in that he was not consulted on decisions involving laboratories shared by both men. Further, he believed that the technical and maintenance personnel considered Waggoner their boss and therefore would not work for Holmes. Holmes said that it was up to Waggoner, he felt, to keep him informed concerning laboratory activities and that he deeply resented Waggoner's attitude in withholding such information from him.

Believing that it would be wise to hear the other side of the story before taking any action, Dr. Paisley called Dr. Waggoner into his office and asked him how he and Dr. Holmes were getting on. Waggoner replied that he had hesitated to bring the matter up, since the relationship was a very strained and unpleasant one. Paisley asked him if he had ever gone directly to Holmes to talk about the differences which appeared to exist between them, or if Holmes had ever come to him. Waggoner said that he had never talked about the situation with Holmes. When asked by Paisley to specify his criticisms of Holmes, Waggoner made the following points: "Holmes works entirely by himself and does not tell me what he is doing. He uses more of the help, space, and facilities than he should. All

he wants to do is sit at a desk and let his assistants do all the work. He doesn't put in a full week's work himself."

After considering these two conversations carefully, Dr. Paisley came to the conclusion that the conflict between the two men was probably a result of the lack of clarification which existed concerning their relative positions. Believing that similar misunderstandings might exist elsewhere in the research department, he addressed a bulletin to the entire research staff as follows:

MEMORANDUM

The research department is divided into functional groups or teams of varying sizes. Sometimes, the size or group may depend on physical factors, but in general it depends on type of work done. Therefore, each group has individual projects and interests but is associated with other company groups in the research department or outside. The facilitation of these associations and coordinating of group activities is one of the research director's[2] responsibilities. Also, determination of research policy and planning of programs so as to obtain reasonable balance and emphasis is also the research director's duty. He must represent these groups to management and see that provision of personnel, supplies, and space is made for the most efficient productivity. He must also be sure unused excess of any of these does not exist. To do the above, the research director must not only receive reports of partially or wholly finished research but must be present at the inception of projects and at all critical decision points along the development of a project.

The group leaders have the same responsibility to other members of their group as the director has to the group leaders. In the same way, it is the group leader's responsibility to keep informed of all activities of his group and to guide them in keeping with overall company policy and specific departmental objectives.

The term "leader" is used advisedly. Senior investigators, capable of working independently, should not be forced to work on projects against their will, but a spirit of cooperation and willingness to consider suggestions is expected of everyone. Of course, all have a certain amount of required routine and bread-and-butter research to do. The project should be considered worthwhile and interesting by all concerned—senior investigator, group leader, and research director.

In the same spirit, the man directly in charge of a project should generate enthusiasm and interest in the work done by his assistants and technicians. This can be done by a little explaining and development of a teaching or helping attitude. Everyone likes to feel he is a necessary part of the show.

E. PAISLEY

The copies of the bulletin sent to Waggoner and Holmes carried the following memorandum of explanation from Dr. Paisley:

FROM: Dr. Eric Paisley
To: Dr. J. L. Waggoner
Dr. C. C. Holmes

As the enclosed memorandum applies to the antibiotic laboratories, Dr. Waggoner is the group leader and Dr. Holmes is a senior investigator. Each has specific assistants assigned to him. The overall operation of the laboratory is Dr.

[2] Whenever the title "research director" is used it means and/or assistant director.

Waggoner's responsibility, and this includes assignment of space and personnel as well as employment and termination of services. It is policy in other groups that individuals capable of independent research have some projects of their own and some jointly. This seems a good idea to me, and I would expect the same between Dr. Waggoner and Dr. Holmes. Frequent, even daily, discussions of problems should be expected between Dr. Waggoner and Dr. Holmes. Professionally, there should be complete frankness and no withholding of work done or intended.

Any additional senior investigators added to the staff will have the same relationship to Dr. Waggoner as Dr. Holmes now has.

E. P.

After sending out the bulletin, Dr. Paisley called in each of the two men in turn to discuss their relationships with one another. To Dr. Holmes he said, in effect, "Look, you know that Waggoner is a quiet sort of fellow who doesn't talk much. I'm sure that many of the times you think he is ignoring you and not taking you into his confidence, it is simply due to the fact that he does not talk a great deal and merely doesn't believe you would be interested. I am sure he has respect for your work and is interested in knowing the work you are doing. I would suggest that you simply go in and talk to him about what you are doing and ask him what he is doing—that way you will be sharing information with each other. Also, you understand that someone must be in charge of the allocation of space and assistants. Dr. Waggoner has been designated for that by me because I can't do it and he has had more experience with such a laboratory than any of the rest of us."

To Dr. Waggoner, the research director said, "I'm sure that when you make decisions on routine lab operations, you consider Dr. Holmes' interests equally with your own, but you never talk to him about these things in advance, and it is probable that he misinterprets many of your actions because you don't explain. He is sensitive about that sort of thing, and in the future I would suggest that you make a point of consulting with him before acting. Why don't you get together at regular intervals, say once a week, and go over routine matters and plans for the future, and exchange progress reports on your work. When I can make it, I'll be glad to join in as a third party."

Dr. Paisley was disappointed to learn that no regular meetings were scheduled following his conversations with the two men; but, as far as he could see, matters between them seemed to be better than before; at least he heard no further complaints. His own contacts with the two men continued on the same basis as before—occasional meetings but mostly routine exchanges with Dr. Waggoner concerning minor problems of lab management. Since Dr. Paisley believed that both men were doing work of good quality, Waggoner and Holmes were given increases in salary at the end of one year's employment with the company. The work of the new laboratory seemed to Paisley to be progressing well in spite of the personality conflict.

About a month after Holmes had received his salary increase and just one day before Dr. Paisley was to depart on an extended business trip, the latter heard indirectly that Holmes was so unhappy about Waggoner that he was seriously thinking about resigning. On the following day, only three hours before his plane was to take off, the harassed research director was still wondering what he should do when a note reached him from Dr. Waggoner. It stated that Waggoner considered Dr. Holmes to be intolerable. Unless Holmes left, the note continued, he himself intended to seek a position elsewhere.

Chapter 5

THE EASTERN ELECTRONIC COMPANY

DR. GORDON GRANDBY, associate director of research for Eastern Electronic, strode energetically into the small conference room where seven of his top scientists were awaiting him. The eighth man in the room was Herb Grinnell, the young engineer who was chief liaison officer to Grandby's group for Cambridge Control Corporation, an affiliate of Eastern's. Grandby greeted the men quickly and nodded to his secretary to close the door.

"Gentlemen," he began, "you are all aware of the general purpose of this morning's meeting even though you have not been briefed on any of the particulars. I don't know all the particulars myself. The only essential fact to have in mind is that we're meeting at the request of Cambridge Control to discuss some possible changes in our liaison procedures."

Dr. Grandby then quickly reviewed the history and purpose of the liaison relationship. In brief, this relationship had grown out of the research laboratory's development of a new concept of automatic control. The laboratory's experimental ACS system, as it was called, promised to be a major breakthrough in the field, and Eastern's executive committee had decided to develop and market it. The research laboratory was charged with responsibility for the design of the system and Cambridge Control with responsibility for its development and manufacture.

"As most of you know," Dr. Grandby said in concluding his opening remarks, "there have been a number of occasions recently in which Cambridge has challenged the effectiveness of our liaison. Consequently, I propose we begin this morning's session by letting Herb brief us on Cambridge's point of view. Then we can throw the meeting open for general discussion and analysis."

"Thank you very much, Dr. Grandby," Grinnell began. "I want to say that I appreciate the fact that all of you have been willing to take time out from busy schedules to discuss this matter. Quite frankly, Cambridge feels that it has assumed some urgency. We don't think for a minute that the difficulties have grown out of hand, but we do feel that they need earnest attention. As best I can, therefore, I will brief you on our point of view. At the same time, I hope you will feel free to interrupt as I go along. In this way we may have every chance to clarify issues and to resolve any differences of opinion we may hold."

"Fair enough, Herb," said Dr. Grandby, "and I'll take you at your word, if I may, to make a prefatory remark. It's quite likely Johnson will not be able to make this meeting, but I want you to know that I spoke with him late yesterday afternoon. He insisted I let you know that he doesn't want his absence to keep you from referring to the discussion that you, he, and I had on the phone last Thursday."

"I appreciate his courtesy, Dr. Grandby," said Grinnell, "and I think the discussion you refer to is an important illustration of the problems we at Cambridge are beginning to encounter. This is a good place to begin."

Grinnell summarized by recalling that Johnson had been at Cambridge Control the week before on a liaison mission from the research laboratory. At the time of his visit Johnson had requested that one of the components he inspected be subjected to further testing under conditions he specified. He also requested that the component design be modified in certain ways and that this modified design be tested in a similar way. Johnson then had said that he would give his approval to the design that had the best test results.

"I'm going to interrupt again, Herb," said Dr. Grandby, "because, as you know, I was involved in the conclusion of this incident. It may make it easier for you and more meaningful for the rest of us to have me tell my side of it."

Dr. Grandby related that the chief engineer at Cambridge had called him on the phone soon after Johnson had left the plant. It seemed, Dr. Grandby explained, that Johnson had committed a procedural blunder by making his recommendations directly to a project engineer without going back through the project head. His action had apparently caused some ruffled feelings.

"Am I telling this right, Herb?" Dr. Grandby asked.

"Yes, you are," Grinnell replied, "but all of you will want to know that from our point of view there was much more to it than ruffled feelings. In fact, the chief was very concerned that no one here knew about the changes Johnson was suggesting. He expressed real concern about your system of authorization when you merely referred him back to Johnson as the authority on that part of the system."

"But all of us knew in a general way," broke in the physicist in the group, "what Johnson was about. We had discussed his line of attack in meetings many times. As a matter of fact, a number of us—and I was among them—had criticized Johnson's approach quite sharply in the beginning, but his logic withstood every attack."

"That's right," agreed Dr. Grandby, "and I said as much to the chief engineer."

"It seems to me," suggested the head mathematician, "that the quarrel here is really a very minor thing. You fellows at Cambridge, Herb, have a close system of control and a sharply defined chain of command. If we

simply adopt a similar system—just as an *ad hoc* administrative convenience—I should think that would solve the basic difficulty."

Before Grinnell could answer, the physicist interrupted to deny that such a change was necessary. He argued that it would waste time and would place an onus of paper work on them that would be a substantial nuisance.

"I don't agree," the mathematician rejoined. "We should be willing to concoct a more formal administrative pattern—again simply as an *ad hoc* measure—if it would enhance our relationships with the engineering groups."

"What's your feeling about this, Herb?" asked Dr. Grandby. "I think we need to get the full Cambridge point of view from you before we attempt any decision making, but we do want to be cooperative."

Dr. Grandby waited for Grinnell to go on, and Grinnell began by stating that Cambridge Control was now in fairly broad agreement with the laboratory on the system itself and that they were prepared to go ahead with the prototype device. Now that the broad outlines had been developed, the procedure at Cambridge was to break up the various parts of the development and to assign them to responsible project heads. These project heads had great authority to budget and develop.

"What you're saying," said Dr. Grandby, "is that now we're in close agreement on the system itself, you've thrown everything up to the project men. You give them responsibility for reaching the objectives, and you'll hold them accountable right down the line."

"That's right," said Grinnell.

"This doesn't seem to be a good approach at all for the development of the ACS system," broke in the physicist in argument. "We've consistently taken a systems point of view in our design, and I think we need to keep a systems point of view now. No other approach makes sense to me. We've got the men who know what they want, and it seems to me they've got to have the responsibility for getting it."

"I don't think anyone at Cambridge is going to deny that the design responsibility is yours," Grinnell replied, "but I do want to stress that within our own organization we have an accountability, too."

"I think it would be helpful, Herb," said Dr. Grandby, "if you would spell out this point a little more fully."

Grinnell explained that each of the project heads had submitted preliminary plans for the prototype device. These plans had been approved at Cambridge Control. They were the same ones that had been sent to the research laboratory three months before and which had been returned to Cambridge with a series of criticisms and suggestions.

"As you know," said Grinnell, "all of your suggestions were thoroughly reviewed by the responsible project heads."

"But not all were tried," interrupted the physicist again. "As a matter of fact, Johnson's difficulty is a good case in point, for some of the tests he

recommended last week were included in his comments of three months ago."

"That's true," replied Grinnell, "but our men felt that they were thoroughly checked out on that component. We've been using it for some time with excellent results and we thought Johnson was being overly cautious about it."

"But the component modifications he suggested last week," said the physicist, "were new to your people. It's quite possible they may make important differences in the reliability of that component."

"I can't testify to that," said Grinnell, "but I can testify to the planning difficulties involved."

Grinnell went on to explain that the approved preliminary plans had now become project plans. The engineers had now begun to submit monthly reports covering costs and work on each project. These reports also covered estimates of future production costs and workload estimates. These reports went upstairs to divisional accounting.

"If Johnson's new component were installed," Grinnell went on, "it would add $10,000 to the cost of each production model. Naturally, the project head wasn't going to stick his neck out. He quite properly felt that he could do it only on the recommendation of some responsible research head. We all feel that the laboratory ought to establish some clear-cut lines of authority in matters of this kind. We also think—and we offer this only as a friendly observation—that it would be helpful to all of us if you would delegate technical authority in a more systematic way."

At this suggestion, the research group broke into disordered discussion until the mathematician spoke up to summarize their feelings.

"The one thing we don't want to do, Herb," he said, "is to smother our attack on the ACS system under rules and regulations that will cut us off from one another. We have always felt that ideas can and should be generated by anyone in the laboratory. We want the system to have the benefit of every technical competence we can bring to bear. We're afraid that if we fetter people with arbitrary procedures we're not going to achieve the breakthrough we're striving for."

"But there's a lot more to it than just rules and procedures," Grinnell continued. "At least, this is our feeling at the plant. We think we've progressed far enough in our work on the system so that we've got to ask for more responsibility in developing it."

"Well, we certainly want your people to have their share of the responsibility, Herb," said the mathematician, "but I think all of us here believe we've got to keep a steady eye on the system itself. This is the important thing—the *sine qua non,* if you will."

"But this is part of our argument," Grinnell countered. "We feel that we have kept the total system very much in mind during this long, formative period. I think you'd agree that we've worked well together and in good harmony; but now the general feeling at Cambridge is that we

must get on with it and that we must assume the responsibility for getting it done. This seems reasonable to us, and surely you will believe that we want the system to be the great success you all think it will be."

"But this doesn't affect our responsibility for the design of the system," said one of the research associates, "and we've still got to have the opportunity to say that everything is as well designed and as well planned as it could be."

"I can certainly understand your point of view," replied Grinnell, "but I must plead with you to understand ours. As much as you're responsible for the design, we're responsible for the execution. And as long as we're charged with completing the project and producing the system, we feel that we've got to have an area of judgment appropriate to determining what's to be done in our area of responsibility."

"And this is exactly our argument, too," said the physicist. "This is the very point of difference we've been debating. Now let us look at this point of difference in a 'for instance.' Let us say, for instance, that Johnson hadn't suggested the component changes on his own initiative: would the project head then have sought him out for advice? I don't think so. Personally, Herb, I feel very strongly that your system of technical accountability stifles initiative. And I worry lest your production-centered philosophy sabotage our chances for success. No one at Cambridge wants to spend the time or money to try for something better than we've got."

"I agree with this, Herb," seconded the mathematician, "and this is important to us. Before we got into this development, we had to convince our executive committee that our concept of control promised to be a major breakthrough. We did this on the basis of our research here in the lab. We're convinced that the ACS system can become everything we've hoped for. However, it will fulfill its promise—and ours—only if we keep giving it everything we've got."

"All of this may sound somewhat selfishly defensive to you, Herb," added one of the research associates, "but you've worked with us long enough to know it's much more than that. You know that we very honestly believe we've got something big here. We know that we can build a reliability into this system that no one thought possible, but we can't cut corners. We've got to spend money and time to accomplish it."

The point, everyone seemed to agree, was that the research group was going to have to pass approval on the final design. It was a matter both of professional pride and managerial responsibility.

"But you can't approve everything at every step of the way," Grinnell countered. He argued, in a courteous way, that the research laboratory was not being reasonable about it. He implied that the whole question of approval seemed to be something of an obsession with the research group. Grinnell also reviewed the history of Cambridge Control. They had

been in the business—and very successfully in the business—long before they had become an affiliate of Eastern's. Many of the components being used in the ACS system were originally developed and engineered by Cambridge men.

"Our whole experience in the field has been good," Grinnell argued. "The components which have been produced by our company are reliable and all the data we get from the field bear this out." Furthermore, Grinnell added, on top of their reliability record, they had a rich pride in their ability to maintain schedules and cut costs. They had a reputation in industry for keeping two steady eyes on the economies of their systems.

"I certainly hope you understand," Grinnell concluded, "that I don't want to build an issue of this. None of us at Cambridge does. But I do want to stress that there is some feeling that the whole question of approval has become urgent now that the basic design is about complete."

"How do you feel about it, Herb?" asked Dr. Grandby.

"Do you mean how do I feel personally or do you mean what do I think is the consensus?" asked Grinnell.

"The latter," replied Grandby.

"Well," said Grinnell, "we respect the fact that the essential design is yours and we want to help you realize your objectives. But we also want you to be fair and helpful in turn. Look again, for instance, at this component modification of Johnson's. If we had made all the changes and run all the tests he wanted, it would have taken two to three weeks out of B project's schedule. Quite frankly, we can't go on this way much longer. We've got to find a means of bringing things to a head."

"But this is only one point of view," argued the physicist. "You should understand that both Johnson and I have grave doubts about the suitability of the component you are using. And it seems to me that we've got to have an opportunity of passing judgment on this kind of thing."

"But you've got to decide that somewhere, sometime, you're going to stop approving," rejoined Grinnell. "We feel very strongly that we've got to get a set of requirements and stick to them."

"But the system alone dictates the requirements," countered the physicist. "Johnson made the decision he did because the B project people hadn't done any more than adapt the old GBQ component to the new system. And our experience with the GBQ was essentially that it was no more than adequate. And we have some doubts that it will be as reliable as it should be for the ACS system."

"Well, I can only repeat again that we *are* concerned with reliability," replied Grinnell. "Perhaps it would be only fair, though, to add that there is a feeling at Cambridge that you fellows want extreme component and system reliabilities. We just can't take up every design idea that comes along. Why, we'd be testing components and arguing theory from here to eternity. At some point you've got to have your project leader say: 'O.K., here's what we do.'"

"But, Herb," interrupted the mathematician, "it's very definitely our feeling that we are running some risk of getting inferior components into the system. After all, ACS will have to have better reliability than anything Cambridge has done before."

"But we have had the experience with the basic units," countered Grinnell, "and we feel that you've got to let us have the practical and basic engineering control over what we do. We know that we're already way ahead of anything that's now in production. But if we don't start moving soon, everybody else will be ahead of us—not on paper, nor in theory, but in real hardware out in the field."

"We sympathize with what you're saying, Herb," said Dr. Grandby, "but we're committed to producing a system that's not going to be one whit inferior to what we know it can be right now."

"Fair enough," agreed Grinnell, "but we've got to reach a point where we can be sure of going ahead. We feel that you've got to do this, for you can't possibly look at every drawing, at every detail. You've got to find some way of giving us final design approval. Believe me, gentlemen, we just want to get on with the job. And we want to do it in a friendly way and to have you make use of our experience and our judgment. Quite frankly, if you will permit me, your opinions have been too fragmented and divided. Somehow, you've got to find a method of arriving at a kind of group approval."

Grinnell leaned back into his chair, obviously finished.

"Herb," said Dr. Grandby in acceptance of his finishing, "we certainly want to thank you for making the Cambridge point of view so clear. Please accept my personal thanks for handling a difficult job as fairly and as honestly as you did."

"Gentlemen," Dr. Grandby added, turning to his associates, "let's take a coffee break. I would suggest we then reconvene here in about ten minutes to see if we can make a start at analyzing some of the problems Herb's presented. Perhaps we can even make a pass at finding some reasonable solutions."

SECTION II

Research Studies

Chapter 6

THE PROFESSIONAL EMPLOYEE IN INDUSTRY*

David G. Moore† and Richard Renck

With the growing importance of technical activities in industry, one might expect that engineers and other professional employees would find considerable satisfaction in their work. They are, after all, at the very center of the nation's industrial effort. Companies compete for their services, newspapers carry stories about the shortages of engineers and scientists, and national advertising glorifies the technological exploits of industry. Under such circumstances it would seem that the technical "brains" of the nation would derive more than average satisfaction from their key position in society. The evidence, however, appears to be quite to the contrary. The technical experts, engineers, and other professionals in industry seem to be far more frustrated than satisfied.

FRUSTRATION AMONG PROFESSIONAL EMPLOYEES IN INDUSTRY

Our observations regarding the frustration of professional employees in industry are based on attitude surveys conducted by the Industrial Relations Center of the University of Chicago. These surveys make use of a standardized attitude questionnaire developed by the Industrial Relations Center and known as the *Employee Inventory*. The *Inventory* consists of seventy-eight statements covering most aspects of work, to which employees may either "agree," "disagree," or remain "undecided." Scores are calculated in terms of percentage favorable response. Results are plotted on profile charts which automatically compare the group surveyed with a cross-section of American employees (see Fig. 1). For each category of the *Inventory* the obtained score of the group is shown in the body of the profile chart and its corresponding percentile value in the black band at the top and bottom of the chart. This permits immediate conversion of obtained scores into percentile values just as in any standard psychological test. Interpretation of the profile is based on analysis of the

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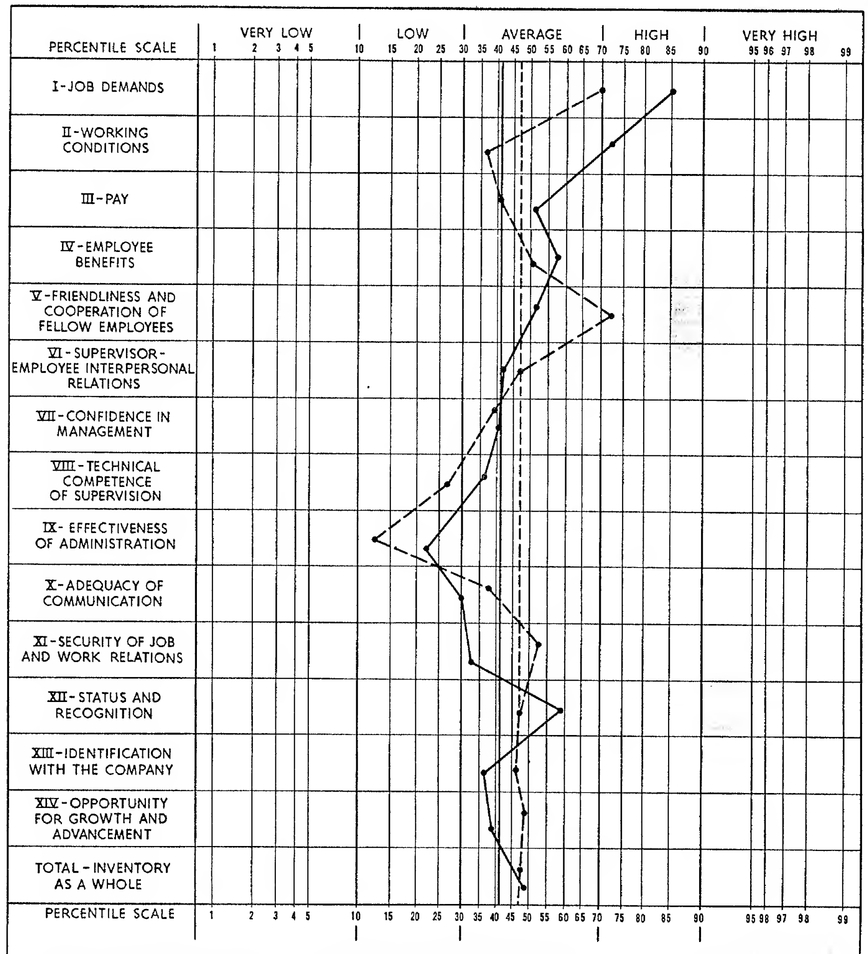
general scoring tendency of the group and on the fluctuations around this general scoring tendency.

Discussion in this report will be limited to engineers and natural scientists, who are more numerous in industry today than other kinds of professional employees, such as doctors, nurses, lawyers, psychologists,



LEGEND: NATURAL SCIENTISTS: ——— NO. GROUPS: 7; NO. PERSONS: 188
ENGINEERS: – – – – NO. GROUPS: 12; NO. PERSONS: 399

FIG. 1. Profile of scores for professional employees.

and artists. Altogether 587 professional employees in nineteen separate work groups are included in our sample. All these represent groups recently surveyed, although most of the trends about which we shall comment have been observed consistently in surveys conducted over the past several years. Of the natural scientists included in our sample, almost all are chemists or physicists, a substantial number of whom are Ph.D.'s.

An unknown proportion of the engineers have graduate training and advanced degrees in their fields.

The outstanding characteristic of the *Inventory* results for both the natural scientists and the engineers (see Fig. 1) was the "Average" scoring tendency (indicated by the perpendicular line based at the midpoint of the category scores). In general, the scoring tendency for any occupational group tends to reflect the status-level of the group in the organization. High-status employees, such as executives, top staff personnel, and other management representatives typically look at the work environment in a more positive way, and score "High" to "Very High" on the *Inventory* profile. Low-status employees, like factory laborers, regard the work situation in a less favorable way and tend to score "Low" to low "Average" on the profile. Whenever the central scoring tendency of a high-status group is lower than what might be expected, it indicates frustration of some kind in the group. It generally means that employees feel that their importance and prestige in the organization are not what they should be.

In the Industrial Relations Center's surveys, professionals revealed only slightly more favorable attitudes than production workers and factory labor. Their feelings appeared to be about the same as routine office employees. They were definitely less satisfied than skilled workers, foremen, salesmen, and management. For high-status employees, their attitudes about the work situation in general were considerably less favorable than might be expected. Furthermore, the trend was consistent from group to group. In practically every professional group which has been surveyed, with the exception of a relatively few cases, there was strong evidence of frustration and generalized dissatisfaction.

The frustration and negative feelings among professional employees can perhaps best be sensed through their written comments which were made during the surveys. As an example, note the bitterness and sarcasm in the following rather long comment which one employee took the trouble to write:

Management in the Research Department does not command my wholehearted loyalty or support because it does not know how to deal with personnel. It does not realize that an honest word of praise for a job well done is appreciated by employees. Persons whose work is not satisfactory frequently do not know that this is the case until they suddenly are discharged from their jobs. Complaints about the boss are summarily ignored—the boss can do no wrong. Working plans for some research projects are drawn only on a day-to-day basis—some projects receive no attention at all. Long-range plans are nonexistent. Little courtesies, like Christmas greetings, congratulations on new babies, etc., are never extended by management. Management does not mingle with workers at parties or elsewhere except when absolutely necessary. Academic degrees are all-important. Ability counts little. For my boss I have only contempt. (Boss-Section leader.) He does not help to plan my work but is free with criticism when the work is finished. Good Monday-morning quarterback. He tries to claim entire credit for work to which he has contributed absolutely

nothing. His knowledge is inadequate. He passes my ideas on to management as his own. He criticizes me to others in our section and comes to me with criticism of others. He is violently critical of others for doing the things he does himself. Rules are only for others, not for him. My boss has inadequate knowledge of our field of work; consequently, he is unable and unwilling to plan or discuss the program to be followed in conducting the work. If we complete a project successfully, he claims the credit; if results are not obtained quickly enough, he disclaims responsibility and criticizes our plan of approach to the work.

This comment is unusual as to length but not necessarily as to tone. The following comments are shorter but illustrate the same negativism and tendency to strike out almost indiscriminately at all aspects of the work environment. In reading these comments, it is necessary to keep in mind that these are high-status, professional employees with college training and, in many cases, advanced degrees.

Supervisory personnel appear to be too interested in protecting their jobs to allow certain type of investigations and thus discourage initiative and imagination in their employees.

I feel that my present immediate superior is not at all qualified to handle men. He maintains the attitude of having to be the first one to think of something or to learn of an event in order for it to be informative or useful. He definitely does not treat his employees with fairness, and he neither gives them proper credit for their work nor does he allow them the opportunity to work freely or independently on a given project.

Morale in company is extremely low. Seems to be a distrust of employees' abilities, and, consequently, management is probably blamed for more than it is responsible for. General disinterest by employees in their work. No clear-cut plan laid down by management for future work. Good ideas but ill used by management, such as merit-rating sheets. Insufficient explanation of raise system and pay bracket. Complaining does no apparent good.

The research department is a disorganized group working inefficiently at a few good programs, many of which are improperly planned, and headed by executives who are lacking in ability to handle personnel. Supervision prefers to "push" rather than lead and often forgets that even technical men are human.

Too much discrimination in expense accounts, promotions, and social status. Too much selfishness by the top engineering bosses. Too much of "I am the boss and can do no wrong" and "Do as I say; you're wrong!"

There is a definite tone of bitterness in these comments which is not unusual for employees who feel that their needs and interests are not being satisfied in the work situation. With attitudes such as these, professionals are undoubtedly a difficult group to supervise. Higher levels of management are apt to feel that they are a "sour" bunch, always complaining about minor matters, picking at details, difficult to motivate, like "pushing against wet bags of sand." This can be especially disconcerting to higher levels of management when the status and educational background of these employees are considered.

There are, of course, groups of professional employees who display considerable satisfaction with the treatment and recognition they receive

in industry. Not all the groups we have surveyed are "Average" or below, but even some groups with "High" morale express a fairly critical attitude toward the effectiveness of administration (Category IX of the *Inventory*).

PATTERN OF ATTITUDES AMONG PROFESSIONAL EMPLOYEES

While the general feeling tone among professional employees is negative, there are specific aspects of the work environment about which they feel more or less favorable. The pattern of attitudes for each group in our sample is revealed by the fluctuations in the category scores above and below the central scoring tendency of the group (indicated by the perpendicular line). Note, for example, the higher score among natural scientists with regard to working conditions. This trend is readily understandable when we consider that many of the natural scientists are working in modern, air-conditioned laboratories, located away from the main plant in a setting often reminiscent of the college campus. The engineers are not quite so fortunate. They are seldom provided with special work facilities and are often crowded into open offices with no privacy or recognition of status or position in the company. Their attitudes are reflected in their score on working conditions, which is only slightly higher than 35 percent of all employee groups surveyed.

Among the natural scientists the category scores are definitely more favorable with regard to job demands (Category I), working conditions (Category II), and status and recognition (Category XII). They are definitely less favorable with regard to effectiveness of administration (Category IX) and adequacy of communication (Category X). There are also trends up and down for several of the other categories which should be noted. In general, the natural scientists gain their greatest satisfaction from the job itself. They like their work and feel that it is important and worth while. These employees are career-oriented rather than company-oriented. They see their futures as lying in the profession rather than in the company. They do not identify strongly with the company, nor do they see any great opportunities in the company for persons with their backgrounds and interests. Professional employees are especially critical of the efficiency of the company and the administrative skills of management. Part of their reaction in this regard might be viewed as the valid criticism of trained observers and analysts who place a high premium on well-conceived plans and orderly procedures for accomplishing them. Part appears to be due to the frustration and chagrin felt by high-status employees whose ideas and opinions do not carry much weight in management circles. Part may be attributed to the difficulties, which appear to be felt very keenly by professionals, of working in an organized setting with a hierarchy of authority and control. Whatever the reasons,

there is a heavy concentration of adverse feelings among professionals regarding the administrative proficiency of management.

Examination of the profile of the engineers reveals a similar pattern of attitudes. However, there are certain variations, one of which has already been noted with regard to working conditions. Like the natural scientists, the engineers tend to be job- and career-oriented, although their feelings are not quite so definite as the natural scientists'. They tend to have more favorable feelings about each other. In short, there appears to be more group solidarity among the engineers than among the natural scientists. Furthermore, they seem to be more secure than the natural scientists. The security of the latter group may be adversely affected by the rather tentative, ill-defined position of research and developmental work in industry. It is not clear to professionals in industry that research is definitely "here to stay." They often feel, rightly or wrongly, that the first item of expense to be cut is the research budget. Moreover, they sense a lack of understanding among higher-level management representatives of the function and place of research in the company. Such attitudes apparently are not characteristic of engineers. The engineers feel that they are "here to stay," although many of them are not sure what their role in the organization is. Note the extremely adverse feelings among engineers with regard to effectiveness of administration (Category IX). On this category, they scored lower than 85 percent of all employee groups surveyed. They view the organization as confused and ill-conceived; they view management as confused and ill-advised. Again, we can view some of this criticism as the valid observation of trained analysts who tend to concentrate attention on "what is wrong" with the organization rather than "what is right" about it. However, a considerable part of the reactions of engineers can be attributed to the feelings of frustration which develop in any work situation where the position, status, and role of employees are poorly defined. As one engineer, an old-timer, put it:

> I can tell you why the engineers around here don't have high morale. This company takes great pride in its engineering and technological achievements if you can believe the ads. But I also know that the executive vice-president has been heard to say on many occasions, "You can hire technical brains a dime a dozen!"

MORALE AND PRODUCTIVITY AMONG PROFESSIONAL EMPLOYEES

Although the findings from the *Inventory* surveys conducted by the Industrial Relations Center indicate a kind of chronic dissatisfaction and frustration among a large number of professional and technical employees, there were two groups in which the trend was reversed. Interestingly

enough, both of these high-morale groups were successful research teams each having developed promising new products for their companies. By the same token, the most demoralized group we have surveyed is one which has failed to gain acceptance for their ideas. Indeed, they regarded themselves and were regarded by others as "failures." A comparison of

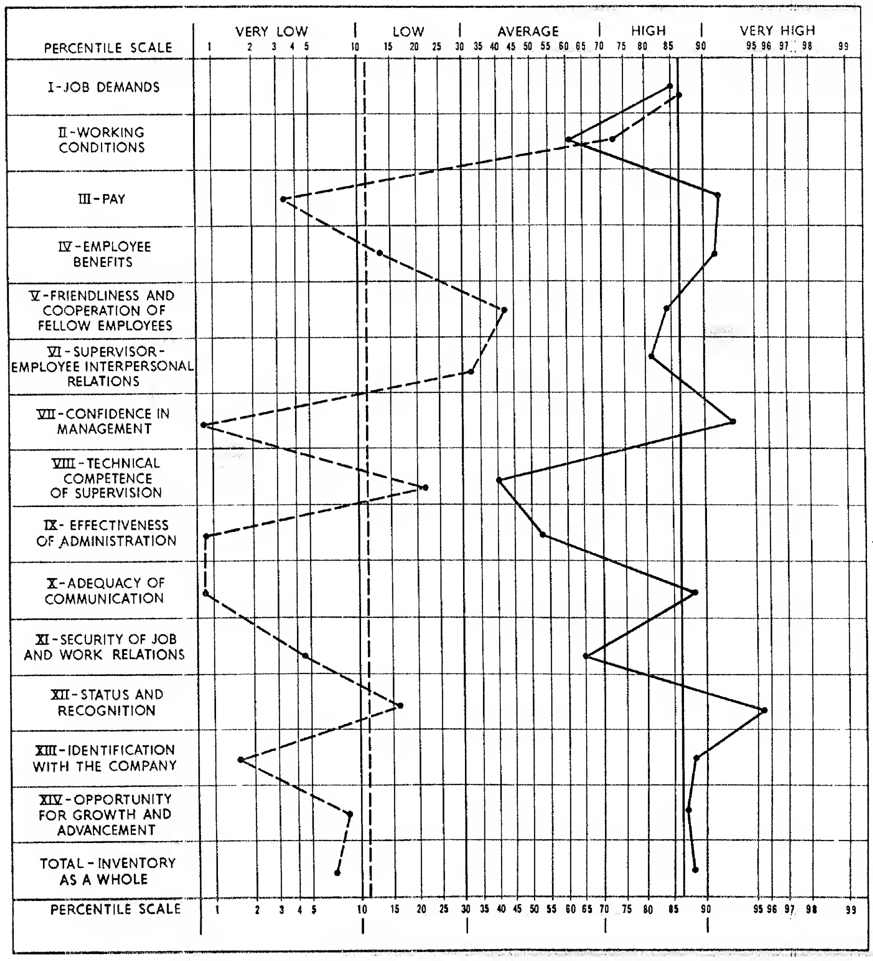


FIG. 2. Profile of scores comparing a successful group ($N = 17$) with an unsuccessful group of natural scientists ($N = 14$).

scores between one of the successful research teams and the very unsuccessful one is shown in Figure 2 and reveals a significant difference between the attitudes of the two groups. While our evidence at this time is only fragmentary, there are indications of a relationship between productivity in research and professional employee morale.

FACTORS IN THE MORALE OF PROFESSIONAL EMPLOYEES

A number of observations can be made regarding the factors influencing the morale of professional and technical employees. These observations are based on case material and scattered information obtained by the Industrial Relations Center's staff in following up on survey results. Conclusions must be regarded as tentative, since no systematic study has been undertaken. It can be stated categorically, however, that the chronic dissatisfaction of professional employees emerges out of a fundamental conflict which exists between the expectations and values of professional employees and the opportunities which they have to realize their ambitions in the industrial setting. Chronic dissatisfaction among any group of employees in our experience is inevitably a result of a conflict or disharmony of this type. It is particularly acute among employees who feel that they should occupy a relatively high status in the organization but find that the rewards and recognition received do not meet their expectations. In such groups, frustration becomes the dominant attitude and often results in aggressive attacks against management and other elements of the organization that appear to be the cause of the frustration. Indeed, it has been our experience that the most "rebellious" groups in industry are frequently higher-status employees who feel that their expected status in the organization is not being achieved or, having been achieved, is in some way threatened.

Tracing through all the elements of this conflict between the values of the professional and the apparently divergent values of the industry of which he is a part is beyond the scope of this paper. However, a few additional observations can be made.

It seems clear from the evidence available that the professional is job-oriented. He is concerned primarily with competent performance in his chosen field. He seeks status through specialization. In a sense, he takes a field which might appear to be extremely narrow to the layman and develops it to the *n*th degree. General management, on the other hand, takes pride in its integrative skills and its ability to move about freely in a variety of fields. From management's standpoint, the specialist often appears to be overelaborating the obvious. You hear such statements in management circles as: "You ask them [meaning the engineering department or research department] a simple question, and you get back a forty-page tome." It is frustrating to management representatives who must try to simplify the environment and cut through a mass of detail to be confronted by specialists who seem equally bent on complicating the environment beyond reason.

From the professional's standpoint, there is often the feeling that management does not know what it is doing. The analytical mind finds the

integrative mind somewhat inconceivable, perhaps even a little obnoxious. The ability to reach decisions on the basis of partial information and what might appear to be flimsy evidence is not held in high esteem by the professional. In fact, in professional circles, this borders on charlatanism.

Because of his job orientation, the professional tends to work within the framework of his field and seeks to extend knowledge within this framework. He proceeds or at least would like to proceed in a systematic, often pedestrian, fashion to develop his field on a broad and complicated front. He is keenly aware of the many facets of his discipline and the total effort required to apply it to the technological and research problems of industry. Management, on the other hand, works within the framework of business. It proceeds of necessity in a more sporadic and opportunistic way, keenly aware of the importance of timing and emphasis to meet the changing demands of the business world. The persistent interest of the professional versus the multiple and seemingly scattered interests of business management often causes a lack of understanding. One engineering group with which we had contact spoke of management's lack of real understanding of the total job of engineering which they felt needed to be done. They complained that management used them primarily as "trouble shooters" and was continuously interfering with the efforts of the department by introducing special requests for work on pet projects.

The entrepreneurial interests of management sometimes come into conflict with the broader field interests of research personnel. Management's concern with a strictly marketable, promotable idea may in certain instances appear superficial to the researcher who sees his own interests as being more fundamental than this. The researcher occasionally gets the notion that he is "prostituting" himself to commercial ends. Management, on the other hand, cannot understand the researcher's inability or perhaps his unwillingness to create new ideas in the entrepreneurial sense. In such instances there may be a tendency for management to depreciate the work of "experts." The view is sometimes expressed in management circles that untrained people frequently come up with the best ideas. In one company a management representative said, "We began to get results out of our research laboratory when we put the sales manager in charge of it. He was just dumb enough not to know that it couldn't be done."

There is some evidence that the patterns of administration and organization which characterize industry and which work reasonably well in most other branches of business are inappropriate to research and technical activities. The number of complaints found in the Industrial Relations Center's surveys regarding the "stealing of ideas by higher levels of supervision" suggests that the "chain of command" which typifies industrial organizations may be a source of considerable tension in the engineering departments or research laboratories. Any job-oriented person seeks recognition for himself. In general, he wants this recognition bestowed upon him for the merit of his work. Indeed, this is one of the

reasons why he is a specialist in the first place. Working in an organized setting in which individual recognition is difficult to secure is not likely to satisfy the personal needs of the professional. More than this, working in an organized setting where aggressive social effort is needed in order to rise in the status system is also a possible source of dissatisfaction. A colleague-type organization with leadership developing more or less informally and based on the ability of the individual as judged by his peers might prove more effective than the typical hierarchical organization.

SUMMARY AND CONCLUSIONS

Because of the central position of technology in modern industry, it would seem that professional employees, including engineers and natural scientists, should be a satisfied, well-integrated group. Evidence from attitude surveys conducted by the Industrial Relations Center of the University of Chicago, however, indicates that these employees tend to be chronically frustrated and dissatisfied. Factors in the morale of professional employees revolve primarily around a fundamental conflict which exists between the expectations and values of professional employees and the opportunities which they have to realize their ambitions and interests as professionals in the industrial setting. "Successful" professional groups apparently have realized their status needs through recognition from and acceptance by higher levels of management.

Chapter 7

VALUE ORIENTATIONS OF MANAGERS AND SCIENTISTS*†

RENATO TAGIURI

SCIENTISTS and managers, when faced with conflicts and misunderstandings with each other, often tend to attribute their problems to differences in value orientations. The conflict is in many instances apparent and has been well documented.[1] The differences in values of the managerial and scientific communities are often very much in evidence and are apt to play a strong role in the relationships between these two groups. Yet some fresh quantitative evidence on values of scientists and managers suggests that the conflict in question needs reexamination. The evidence on the conflict has been based partly on research, where it has been assumed that what people say about their own values and those of the other group reflects the *actual* situation. There is now considerable evidence to support the idea that information derived in this way is not sufficient for an adequate treatment of this problem.

In the process of differentiating themselves from management, scientists may tend to see managers in a manner distorted by the tensions in the relationship. They may also develop a concept of their own values which is likely to be correspondingly distorted. Managers may tend to do the same thing with regard to industrial scientists. Each side winds up with images that, to a considerable extent, are deformed by the tensions existing between the two groups. These images, therefore, may be quite misleading even though they are widely and even mutually accepted, to the point of becoming stereotypes.

For several years the author has been giving attention to this problem,

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† This paper is a condensation of a monograph in preparation by Renato Tagiuri, based on an investigation supported by the Division of Research of the Graduate School of Business Administration, Harvard University. The cooperation of Professor Bertrand Fox, Director of the Division, is gratefully acknowledged. Some of the material appeared in Chapter 20 of *Managers and Scientists*, by Ralph M. Hower and Charles D. Orth (Boston: Harvard Business School, Division of Research, 1963). The author is indebted to them for many ideas and suggestions.

[1] See especially R. M. Hower, and C. D. Orth, *Managers and Scientists*. Chapters 3, 4, 5, and 6 are particularly pertinent.

accumulating data on the values of three groups: scientists, men who achieve executive positions through traditional routes (manufacturing, sales, finance, etc.), and yet other executives who began their careers as scientists or engineers. These data, to be discussed in detail, confirm that the value *differences* mentioned above do exist, but suggest that they may be less great than generally believed. Indeed there are *similarities* in value orientation among these three groups of men that are often overlooked.

Procedure and Subjects

Let us describe briefly how the data were obtained and the groups of subjects studied.

Value here is viewed as a "conception, explicit or implicit, distinctive of an individual or characteristic of a group, of the *desirable* which influences the *selection* from available modes, means, and ends of action."[2] Operationally, values will be defined as the scores obtained by means of the Allport-Vernon-Lindzey "Study of Values"[3] questionnaire, the instrument used in this investigation.

The "Study of Values" questionnaire is based on a well-known typology proposed by the philosopher Edward Spranger. Spranger found it useful to distinguish among six kinds of value orientations and, in terms of these, he described six primary hypothetical types of men: the *theoretical*, the *economic*, the *aesthetic*, the *social*, the *political*, and the *religious* types. Allport and Vernon constructed the questionnaire on the basis of Spranger's theoretical work and later revised it in collaboration with Lindzey. This test has been used for some 30 years in a number of ways and is regarded as a very useful measure of value orientations and interests.

The proper interpretation of the results of this test requires some technical knowledge of the instrument itself. Most important is a clear understanding of the labels used to describe the six values. These terms can be very misleading if understood in terms of their usual dictionary meaning. The *social* value, for example, does not refer to an interest in being together with people but, rather, to altruism. To clarify this matter, a description of what each of the six scales purports to measure is given below.

1. *The Theoretical.* The theoretical man is primarily interested in the discovery of *truth*, in the systematic ordering of his knowledge. In pursuing this goal he typically takes a "cognitive" approach, looking for identities and differences with relative disregard for the beauty or utility

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[2] C. Kluckhohn, "Values and Value-Orientations in the Theory of Action," *Toward a General Theory of Action* (Parsons and Shils, eds.), (Cambridge, Mass.: Harvard University Press, 1951).

[3] G. W. Allport, P. E. Vernon, and G. Lindzey, *Study of Values* (3d ed.; Boston: Houghton Mifflin Co., 1960).

of objects, seeking only to observe and to reason. His interests are empirical, critical, and rational. He is an intellectualist. Scientists or philosophers are often of this type.

2. *The Economic.* The economic man is primarily oriented toward what is *useful.* He is interested in the practical affairs of the business world, in the production, marketing, and consumption of goods, in the use of economic resources, and in the accumulation of tangible wealth. He is thoroughly "practical" and fits well the stereotype of the American businessman.

3. *The Aesthetic.* The aesthetic man finds his chief interest in the artistic aspects of life, although he need not be a creative artist. He values *form* and *harmony.* He views experience in terms of grace, symmetry, or harmony. Each single event is savored for its own sake.

4. *The Social.* The essential value for the social man is love of people: the altruistic or philanthropic aspect of love. The social man values people as ends, and tends to be kind, sympathetic, and unselfish. He finds those who have strong theoretical, economic, and aesthetic orientations rather cold. Unlike the political type, the social man regards love as the most important component of human relationship. In its purest form the social orientation is selfless and approaches the religious attitude.

5. *The Political.* The political man is characteristically oriented toward *power,* not necessarily in politics, but in whatever area he functions. Most leaders have a high power orientation. Competition plays a large role in all life, and many writers have regarded power as the most universal motive. For some men this motive is uppermost, driving them to seek personal power, influence, and recognition.

6. *The Religious.* Unity is the dominant value of the religious man. He has a mystical orientation and seeks to relate himself to the universe in a meaningful way. As Spranger puts it, the religious man is one "whose mental structure is permanently directed to the creation of the highest and absolutely satisfying value experience."

So much for the meaning of the six-value dimensions. Consider next the *scoring system* of the questionnaire. It is so designed that the total for the six values is always 240 points; it is the *distribution* of these 240 points over the six values that matters. What this questionnaire shows is the *relative* importance the six values have *within a particular individual's value system.* This scoring system must be understood clearly because it limits the kinds of interpretations that can be made on its basis. It is possible, for example, to point out that Mr. X gives more emphasis to the economic value than Mr. Y, each relative to his other values. It is not legitimate to infer from this questionnaire, no matter what the scores are, that Mr. X has a stronger (in absolute terms) economic value orientation than Mr. Y.

Regarding the *subjects* of this inquiry, three groups are involved:

IRI's are R & D executives who attended the Industrial Research

Institute R & D management seminars at Harvard University in 1961, 1962, or 1963, and who filled out the questionnaire under various sets of instructions. There are 178 such men. These men occupy a role which is quite different from both scientists and the more usual type of business manager. In many ways these men could be pictured as "men in the middle," between these two other groups. Indeed, this is one of the things we will seek to confirm in looking at the data.

Scientists are men who have been scientists in industry for at least seven years with no management responsibilities except supervision of research assistants. Upon request by the 1962 IRI's, 71 of these men filled out the questionnaire and sent it directly to the investigator.

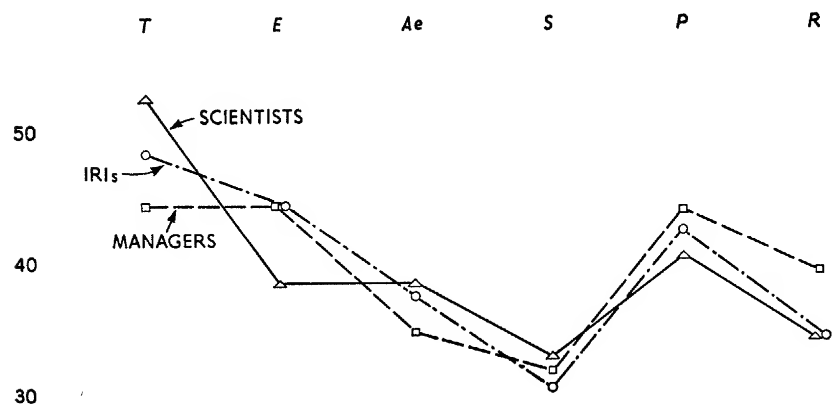
Managers are 368 American businessmen who attended the Advanced Management Program (AMP) at Harvard University in 1960, 1961, or 1962, and filled out the questionnaire as part of a course given by the investigator.

The data, it is important to know, were collected under conditions that would minimize a respondent's natural tendency to try to show up well in terms of some ideal existing in his own mind. All subjects filled out the questionnaire anonymously, during a seminar on value orientations, for the purpose of seeing for themselves where their own values stood in terms of this particular device. Their main motivation for answering the questionnaires was curiosity about themselves.

Exhibit 1

VALUES OF IRI'S, SCIENTISTS, AND MANAGERS

(MEANS. 1951 NORMS)



Results

Exhibit 1 shows the data on the mean value scores. The results reported in this paper are pooled from three independent sets of data collected over a period of several years. The relationships are the same in each of the sub-

sets of data and they can be regarded as cross-validated twice. Thus the likelihood that what we see here is a set of random relationships is extremely small.

Neglecting the actual means for the moment, and considering only the relative order of the six values, as shown below, it appears that there are no substantial differences between any of the three pairings that can be made with the three groups of subjects.

Scientists	*IRI's*	*Managers*
Theoretical	Theoretical	Theoretical
Political	Economic	Economic
Economic	Political	Political
Aesthetic	Aesthetic	Religious
Religious	Religious	Aesthetic
Social	Social	Social

All three groups have relatively high *theoretical*, *economic*, and *political* values, and relatively low *aesthetic*, *religious*, and *social* values. It may surprise the reader to find the *economic* and *political* dimensions among the higher values for the scientist, and the theoretical among the higher values for the managers. Thus the managers appear to have a stronger interest, relative to their other values, in abstract ideas, in the "empirical, critical and rational" than is popularly believed, while the scientists reflect a relatively stronger orientation than might be expected in what is useful, in the production, marketing, and consumption of goods, as well as in personal power, influence, and renown.

On the other hand, Exhibit 1 shows that the scientists' values are relatively undifferentiated except for the dominant and outstanding *theoretical* value, where they differ substantially from the managers, with their emphatic stress on ideas. The managers, in turn, have a tight high cluster consisting of the *theoretical*, *economic* and *political* values, which reflects their special simultaneous orientation in the realms of ideas, utility, and power. The data do tend to give some support to the presumed value clash between scientists and business managers. Certainly the *economic*, *political*, and *religious* scores of executives are higher than they are for scientists, and the *theoretical* is dominant over the relatively undifferentiated other values of scientists.

The IRI's values tend to be in between those of scientists and managers, and show both their dominant orientation toward ideas (*theoretical*) and, simultaneously, the relatively high interest they place upon the *economic* and *political* values; that is, on what is useful, on power, and on influence. These data support the idea of the R & D manager (IRI) as the "man in the middle"—where, indeed, they would seem to belong. If their function is the conciliation of the other two groups, their value orientations are appropriate. This does not necessarily mean that these men are changing from a scientific to a managerial orientation. To many of these men, this "middle" position may be appropriate, given their values. Some of these

men may be in the positions they are *because* of the values they hold rather than vice versa.

The main point about these comparisons is that while the profiles of these three groups of men vary in the differentiation of their value systems, the relative order of their values is quite similar. The high relative *theoretical* value of scientists tends to obscure some of the similarities with the other two groups.

Not much can be made of this observation except to note that these results do not fit a hypothesis of *complete* value conflict as, for example, would be illustrated by a comparison between the IRI's and a sample of respondents in the field of religion, whose values are shown below in descending rank order.

IRI	*Religion*[4]
Theoretical	Religious
Economic	Social
Political	Aesthetic
Aesthetic	Political
Religious	Theoretical
Social	Economic

The data presented here, on scientists, IRI's, and managers, it must be kept in mind, are averages for groups of men. Since the samples are fairly large, these means are quite reliable. However, great individual variations in value orientations exist within each of the samples, and it is not difficult to find in the data available instances of almost completely contrary profiles. Thus, it is easy to see that in many instances severe conflicts must exist and it is probably these that draw the greatest attention.

More useful, perhaps, for an understanding of the relationship between scientists, managers, and their intermediaries, the IRI's, are the data to be discussed next, on the values IRI's attribute to the other two groups. Exhibit 2 shows the values the IRI's attribute to scientists and managers. These data were obtained by asking the IRI's to put themselves in the shoes of such men and answer the values questionnaire as these other men would.

The data in Exhibit 2 indicate that, by and large, IRI men are well aware of the relative values of scientists and executives. There are, however, discrepancies worth noting and perhaps worth taking seriously, in view of the fact that these results were replicated on three difference samples, here pooled for convenience.

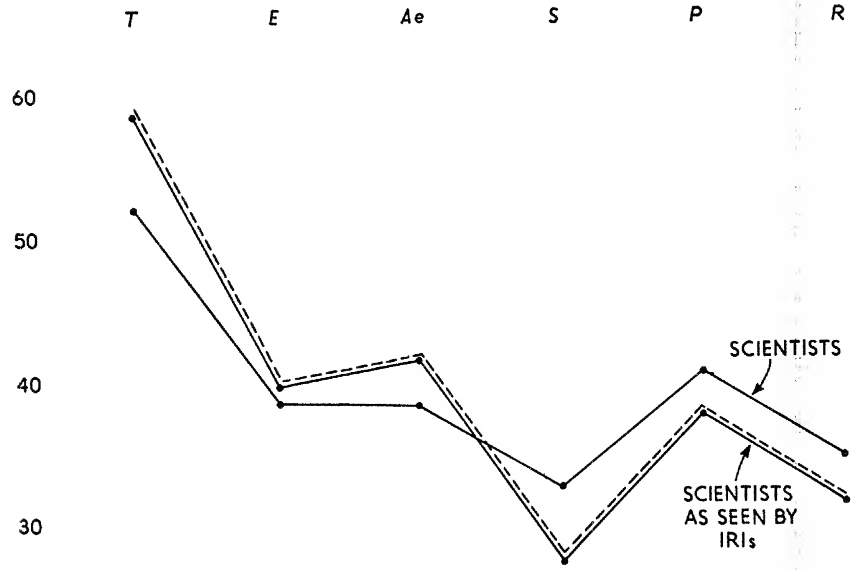
The IRI's perceive the scientist and the executive as much more differentiated (note the spread) in their values than they actually are. The scientist (Exhibit 2A) is seen as having the *theoretical* value as even more highly dominant than it is already over two increasingly lower clusters: the *aesthetic, economic, political,* and the *religious, social.* In actuality, the

[4] Adapted from page 14 of the *Manual for the Study of Values,* see footnote 2.

Exhibit 2

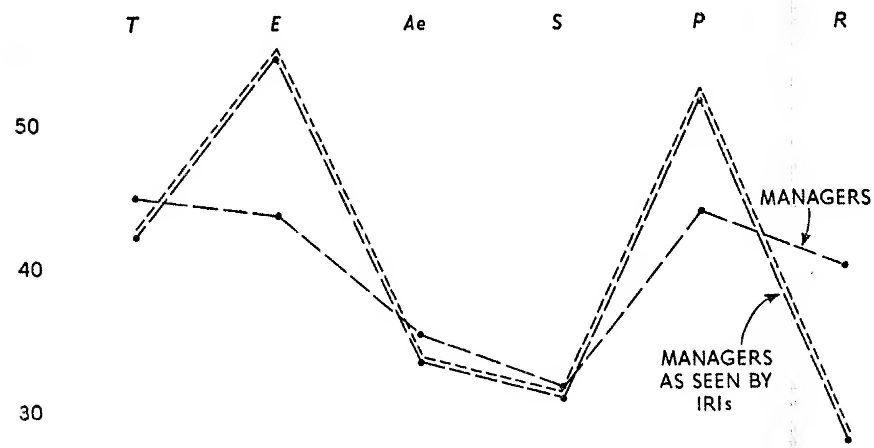
A. VALUES OF SCIENTISTS: ACTUAL AND AS ATTRIBUTED TO THEM BY IRI'S

(MEANS. 1951 NORMS)



B. VALUES OF MANAGERS: ACTUAL AND AS ATTRIBUTED TO THEM BY IRI'S

(MEANS. 1951 NORMS)



scientists' values, except for the *theoretical,* all cluster quite closely, but roughly in the order perceived by the IRI's. His *theoretical* and *aesthetic* (and *economic*?) values are overestimated. His other values are underestimated. The manager (Exhibit 2B) is perceived as having clearly more

differentiated values than he has, with dominant *economic* and *political* values, whereas in fact these are not as "far" from his other values as imagined. The relative positions of his *theoretical* and *religious* orientations are underestimated.

In their perceptions of both scientists and managers, the IRI's stretch or magnify what would otherwise be a fairly accurate picture. The implications of this for the relationships the IRI-type executive has with scientists on one side and management on the other are worth exploration. We have already seen that the IRI's actual values have a configuration that is intermediate to those of scientists and managers. Now it seems that, in the IRI's eyes, this in-between position is seen in relation to magnified ideas of the *differences* between scientists and managers, yet the gulf between these two groups is not as wide as it is assumed to be by their intermediaries.

Discussion

If these tentative conclusions complicate the problem of understanding the conflicts between scientists and managers, they also provide grounds for hope that their relationships can improve. Some of those who have dealt with the subject in the past may have used evidence that exaggerates the differences and overlooks some of the similarities upon which better understanding can be built. For here there is some quantitative evidence that the gulf between managers and scientists in industry is not as great as it is generally thought to be, and that some of it is imaginary in the literal sense of the word.

The mediator role between scientists and managers is, without doubt, one that includes great stresses and strains, for the two groups clearly have different immediate and practical needs. As they position themselves for mediation by administrators of the IRI type, both scientists and managers may present for view simpler and more extreme behaviors than in fact reflects their true values. Thus the mediator may receive a distorted view. It is also likely that the mediator himself may "clarify" his own role and better understand and cope with the stresses of his position by further magnifying the discrepancies of the value positions of the two groups. In the process of communication that ensues through the mediator, these perceived discrepancies may be presented as reality and, in feedback fashion, end up by actually increasing the discrepancy.

This hypothesis may be worthy of further thought, at least by those who feel that such differences are disfunctional to the collaboration of these groups. The trend of events, however, favors a further approachment of the actual values of these two groups as well as of the values attributed to them by yet other influential groups, such as the managers of scientific personnel, for during the past generation there has been an increased emphasis within management itself upon scientific approaches

and a professional point of view. The rapid development of computer technology as an aid to management decision making, the managerial experiences gained by scientists in the rapidly growing research and development non-profit organizations, and the increasing opportunities for university scientists to come in contact with "outside" organizations all seem to converge to decrease, rather than increase, the differences.

Chapter 8

*ORGANIZATIONAL SYSTEMS AND ENGINEERING GROUPS**

LOUIS B. BARNES†

THIS is a comparative study of the behavior of engineers and technicians in two engineering groups located in two different companies. More specifically, it is a study of how management attitudes interact with the reference group affiliation of technical employees to result in two different social systems. The two groups will be discussed in terms of differences in communication patterns, nonwork activities, friendships, performance, satisfaction, and status. We will first present data on attitudes, outlooks, and patterns of behavior for the two groups (which have been labeled Department A and Department B). A final section of this paper will include a discussion of the implication of this study for management.

DEPARTMENT A

Departments A and B both employed about 30 full-time engineers, technicians, and clerical assistants. Both departments were engaged in product development work which involved technical assignments ranging from highly routine to highly challenging. Company A, a large subsidiary of a still larger electronics firm, was at the time of the study engaged in a highly competitive market.

In Company A, management tended to stress management control and to limit engineer autonomy, interaction opportunities, and influence, as compared with Company B. In essence, Company A's management emphasized the goals of productivity and practicality. In comparison, Company B's management tended to encourage subordinate autonomy, interaction opportunities, and upward influence relative to Company A. The goals of productivity and practicality were strongly influenced by the B company's long-standing concern for individual rights and individual development.

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* Copyright, 1960, by the President and Fellows of Harvard College. Excerpted from *Organizational Systems and Engineering Groups*, Division of Research, Harvard Graduate School of Business Administration, 1960.

† Harvard Graduate School of Business Administration.

As a result, Department A members worked in a system which was relatively *closed* around them and which resisted their efforts to change it. They were expected to adhere to management's values without making attempts to influence these values. In comparison, Department B's members worked in an organizational system which was *more open* to their needs and influence, or, more exactly, Department B members worked within a *relatively open system* compared with Department A's *relatively closed system.* In both departments, group members undoubtedly influenced their superiors and management. Company B's management, however, acknowledged and encouraged the feedback process. Company A's management tended to discourage it.

The stiff competition Company A faced and the resulting pressure from its parent company led the general manager of Company A to put pressure on his subordinates for low costs and high quality. This pressure, combined with the pressures arising out of management's attitudes about employees and their work, tended to force the chief engineer of Company A into a "man-in-the-middle" role. Faced with a different set of pressures from his subordinates, he found himself trying to communicate his subordinate's attitudes on science, research, and the importance of engineering to his superiors in management, while at the same time attempting to communicate his superior's concerns for productivity and practicality to Department A and its supervisor. Inadvertently, however, he discouraged development work and encouraged factory relationships for Department A's engineers.

The supervisor of Department A displayed this same "man-in-the-middle" behavior. When speaking to the chief engineer or to management, Supervisor A pleaded the values of science and stressed the search for knowledge and its orderly development. When speaking to his subordinates, Supervisor A displayed a dominant concern for productivity. In playing these expected roles, both the chief engineer and Supervisor A conflicted with superiors and subordinates alike. Supervisor A's pleas for scientific rigor were met by the chief engineer's demands for greater productivity and practicality, and this same pattern was repeated at a higher level.

Correlation of Employee Reference Groups with Observed Behavior

In line with and supporting the points made above about management's role conflict in the engineering division of Company A, the research also showed that the employees in Department A could be divided into three different "reference groups." In order to facilitate discussion of these groups, the author labeled them *Professionals, Organizationals,* and *Socials,* and distinguished them as follows:

Professionals talked as though they identified themselves with the values of science. *Organizationals* talked as though they identified themselves with man-

agement and the values of business. *Socials* talked as though they identified themselves with the cultural values of friendliness and sincerity represented by family relations, social consciousness, and religious beliefs.[1]

Group	*Theoretical*	*Economic*	*Aesthetic*	*Political*	*Social*	*Religious*
Professionals	49.7	35.8	36.9	41.2	36.0	40.0
Organizationals	47.4	44.2	30.4	39.9	38.6	39.9
Socials	39.7	41.8	31.6	40.7	42.6	44.7

Relationship between Reference Group Affiliation and Communication Patterns

Exhibit 1 shows the relationship[2] between reference group affiliation and interaction frequency (the number of times the researcher observed the person talking to or listening to another person during designated observation periods over a five-week period). It will be noted that the younger Socials interacted with others in the department most frequently, followed in order by Organizationals, older Socials, and Professionals. It was also found that the number of different people a person was observed interacting with followed the same reference group pattern.

Exhibit 1

REFERENCE GROUPS AND FREQUENCY OF INTERACTIONS IN DEPARTMENT A

(Total Contacts ÷ Counts in Department*)

*Median Frequency of Inter-actions**	*Reference Group Categories*	*Number of Individuals Engaged in High Inter-action*	*Number of Individuals Engaged in Low Inter-action*	*Total*
.40	Professional	4	7	11
.59	Organizational	6	3	9
.71	Social (Younger)	4	1	5
.46	(Older)	1	4	5
	Total	15	15	30

* One hundred and thirty-nine interaction counts were made on different days over a period of five weeks. On any given day, observations were made at 15-minute intervals. Total contacts reflected the fact that some observed interactions were with two or more people.

Correlation between Reference Group Affiliation and Participation in Nonwork Activity

It also proved possible to correlate reference group affiliation with participation in nonwork activities in Department A. Nonwork activities

[1] Editorial Note: The existence of these reference groups was not only substantiated by extensive interviews with the employees of Department A but also by scores on the Allport-Vernon-Lindzey Test of Values described in "Value Orientations of Managers and Scientists," beginning on p. 63 of this book. For those who are interested in this test, the scores of these three groups were as follows:

[2] These data can be deceptive: for example, three of the four high-frequency Professionals interacted almost entirely with one another around work-related problems. None of the four Professionals who ranked high in Exhibit 1 ranked high with respect to the number of different people a person interacted with.

were divided into two categories called "limited" and "extended." Limited nonwork activities included those involving interactions of relatively short duration such as (1) coffee preparation and (2) baseball pools. Extended nonwork activities included longer duration activities such as (1) coffee groups, (2) lunch groups, (3) horseplay and joking, (4) special parties and get-togethers.

In order to find out which individuals participated in which activities, these informal groups and their make-up were observed over an extended period of time. Exhibit 2 is a sample of the kind of data which was obtained from these observations. In this instance, it can be noted that Organizationals and Socials were almost all involved in lunch groups, whereas only a third of the Professionals were so involved. We can, in fact, summarize the rest of the nonwork data by noting that Professionals tended toward low participation in both limited and extended nonwork activities.

Exhibit 2

REFERENCE GROUPS AND LUNCH GROUPS IN DEPARTMENT A

p[P <0] = .025

Reference Group Category	*Participants*	*Nonparticipants*	*Total*
Professional	4	7	11
Organizational	8	1	9
Social (Younger)	5	0	5
(Older)	5	0	5
Total	22	8	30

Correlation between Reference Group Affiliation and Friendship Patterns

Not surprisingly, involvement in friendships—in terms of (1) friendship choices made, (2) friendship choices received, and (3) mutual friendship choices—tended to be consistent with the data on nonwork activity participation. Professionals were lowest in friendship involvement; they averaged fewest outgoing, incoming, and mutual friendship choices. Organizationals tended toward the highest average number of outgoing and mutual friendship choices among the three reference groups' members, and they also received a high number of choices.

Correlation between Reference Group Affiliation, Job Performance, and Satisfaction

Evaluations were obtained from interviews with management, outside departments, customers, Supervisor A, and the group members themselves. With regard to group performance, the chief engineer reported that

"The company has confidence in Department A's employees as hard working and capable." At the same time, the chief engineer complained that "The backgrounds and knowledge of Department A are not being as well utilized as they might be. Department A engineers should make their specifications and recommendations more practical. They need to get back into the main stream of things."

Outsiders also evaluated Department A's performance record in somewhat negative terms. Typical of these comments was one made by an engineer who had once worked in Department A:

> As far as the production people are concerned, Department A probably does excellent work and highly sophisticated engineering, but from the point of view of other engineering groups, Department A does work of routine quality, much of which we can't count on. Only a few of their engineers have reputations of real scientific integrity.

Regarding individual job performance, Exhibit 3 relates reference group affiliation to engineering performance as evaluated by Supervisor A.[3] The average rank position of Professionals was clearly higher than the

Exhibit 3

REFERENCE GROUPS AND ENGINEER JOB PERFORMANCE IN DEPARTMENT A

Reference Group Category		*Number of Engineers* *High Perform-ance*	*Low Perform-ance*	*Total*
Professional	N = 8	5	3	8
Organizational	N = 9	2	7	9
Social	N = 1	0	1	1
Total		7*	11*	18†

* As close to median as possible. No closer due to tied rank positions.
† Nineteenth engineer not rated; too new in company.

average rank position of either Organizationals or the one Social (a female engineer). These data suggest that the Professionals, those who identified with the values of science, received higher performance ratings than Organizationals, whose values were more in line with management's. Herein lies a real paradox: engineers least identified with management's value system received the highest performance evaluations from management. As noted earlier, the chief engineer and Supervisor A wanted Professionals to adopt more Organizational-like values. The question naturally arises: would the reduction of these Professional values help engineering performance levels and/or management's goals in Department A? The evidence would suggest a negative answer to this question.

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[3] Supervisor A felt unable to distinguish adequately between relatively high and relatively low technical performance. The few technician evaluations he did make were all low rank scores.

Satisfaction[4] was also found to correlate with reference group affiliation. Professionals were less satisfied than either Organizationals or Socials.

Status Patterns

Professionals tended to rank higher than Organizationals on *all* status dimensions. In other words, all the factors denoting a man's status were congruent or in line with each other for engineers in Department A. These data suggest that Department A members identified with that reference group chosen by others in the department who most closely resembled themselves. Reference group affiliation therefore apparently reflected both status similarities and reference group values.

Certainty and Closed Systems

Department A's status, reference group, and social structure patterns present pictures of certainty and clearly defined relationships. Department members who shared status levels tended to identify with the same reference group and to assume similar positions in the departmental social structure. In other words, it was easy to know "where a person stood" in Department A; his status position was relatively clear. Such certainty has several implications. First, it suggests that certainty was achieved in Department A at the expense of flexible interpersonal relationships. An individual found it difficult to break out of the limits imposed upon him by his status background.

Secondly, such high status congruence and certainty of position suggests that some Professionals who might one day have aspired to Organizational promotion found their hopes dimmed as they gained age and seniority. Interview material supported this role transition theory in Department A. Some Professionals talked as though they had adopted Professional values as they grew older, gained higher competence, and were not promoted either to section leader or out of the department. For younger, lower paid Organizationals, hope still remained. They could still aspire to management success. They signified these aspirations by identifying with Organizational values and by becoming regular members of the department.

Although Department A members seemed highly bound by status and reference group affiliations, there were a few exceptions. The most notable was an engineer considered "best liked" by department members.

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[4] Measured on a firm point scale with regard to (1) the work itself and its challenge, (2) pay, (3) associations with other people, (4) working conditions, (5) direct supervision, (6) company management and reputation, and (7) overall satisfaction. The overall satisfaction score (item 7) was multiplied six times to give it a weighing equal to the other six factors.

Interestingly enough, his overall status position in the total group was ambiguous. Some factors were high, others low; a condition of status incongruence existed. This ambiguous status permitted him to identify with either Professionals or Organizationals; he had apparently sacrificed the security of strict identification in order to straddle both reference group and social structure lines. He thus became an important informal link within the department and between Department A and other outside departments.

Several questions are posed by the deviancy of this informal leader. The interview data suggest that he played an important role in the satisfaction and performance of department members. He definitely served as a buffer and a link between engineers and technicians and the supervisor. He was extremely active in the department's helping relationships. And yet his behavior did not resemble the typical behavior patterns of other departmental members. Why? If the role was so useful, what prevented others from playing it? Could management provide a context where individual behavior responded more to actual needs than to status and reference group backgrounds?

At this stage, the author consciously ran the risk of oversimplification by referring to Department A as a relatively "closed" system. Members seemed to respond more to their own status and reference group positions than to the problems in a given situation. Individuals apparently became status-oriented and/or reference-group-oriented, rather than situationally oriented. Furthermore, management's goals and behavior tended to discourage a situational orientation. Instead management pressed for its constant goals of productivity and practicality.

DEPARTMENT B

Compay B was an independent organization, manufacturing and selling electro-mechanical apparatus. It employed over 2,000 people who lived either in the same community or in nearby towns or suburban communities. Company B was larger than Company A, but considerably smaller than Company A's parent organization. Like Company A, Company B was engaged in a highly competitive field. Competition was somewhat minimized, however, by management's choosing to compete only in product areas where the company had a distinct technical advantage. Within Company B the author studied a second engineering department, referred to as Department B.

Management Attitudes in Company B

Statements suggested that management values in Company B helped to structure a relatively *open* system within which work groups avoided the downward influence of required activities, interactions, and sentiments

that took little account of informal influences. Company B management talked as though they stressed high individual autonomy while encouraging interactions beyond those required by the formal task. In short, management adopted a value system whose openness to influence could draw together rather than split apart the different needs of the organization and its employees.

In Department B, no strong value conflict existed between defenders of knowledge for its own sake and defenders of organizational values. The conflict never materialized, partly because the organizational system reduced any reasons for conflict. Without resorting to the more abstract value system of science, both management and Department B employees placed a high value on situational knowledge.

Although the head of field engineering worked in a situation where management chose not to exert strong pressures for productivity and practicality, he had adopted management's value of individual development. Moreover, both he and Supervisor B had apparently made it operational and workable within his engineering groups. In effect, the head of field engineering described himself as trying to structure (1) areas of high autonomy for subordinates, (2) opportunities for interaction between himself and subordinates, and between the subordinates themselves, and, related to these, (3) a system of mutual influence between himself and his subordinates.

Unlike the chief engineer in Company A, Company B's head of field engineering seemed relatively free from explicit management pressures for productivity and practicality. In addition, the reference group differences between science and the organization posed, for him and for Supervisor B, no particular problem. In Company A these same differences had placed both the chief engineer and supervisor A in difficult man-in-the-middle positions between their subordinates and their superiors.

Correlation of Reference Groups with Observed Behavior

In Department B interviews and questionnaires with both management and group members failed to show the split between science and business values found in Department A. Only two Department B members identified themselves strongly with either the values of science *or* the cultural values of management and business. Other department members tended to identify themselves with both and/or with outside familial or community interests.

Communication Patterns in Department B

Exhibit 4 shows the median interaction levels for both Departments A and B along the dimensions of (1) total interaction frequency within the department and (2) range of interactions within the department. This

exhibit shows that in both cases interaction was higher in Department B than in Department A.

The data also showed that there were differences in interaction patterns between the supervisors of the two departments and their subordinates. For example, Department A members had 57 interactions with Supervisor A while interaction counts were being taken. Department B members had 66 interactions with Supervisor B, even though Supervisor B was out sick for 32 of the 139 counts.

The data not only showed a higher frequency of interaction between Supervisor B and his subordinates, but also a wider range of interaction.

One explanation for Supervisor A's lower interaction with his subordinates lies in the organizational structure of Department A. Supervisor A delegated supervisory responsibilities to his five section leaders. By contrast, Supervisor B had all engineers reporting to him and all technicians reporting to the technician foreman. Department B employees believed that such an organizational structure gave each person an equal opportunity to interact with the formal leader. Equally important, these conditions provided a structure within which each engineer could exert influence upon the supervisor.

Exhibit 4

TOTAL INTERACTION FREQUENCY AND RANGE OF INTERACTION MEDIANS IN Departments A and B

	Median	
Department	*Total Interaction Frequency (Total contacts ÷ counts in dept.)*	*Range of Interaction with Members of Own Department*
Department A	.54	14 (47%)*
Department B	.64	17 (53%)*

* Percentage of possible interactions within department.

Friendship and Nonwork Activity Patterns

The very different organizational patterns found in Departments A and B had little effect on the total number of choices in each department. Data from both departments were essentially similar. At the same time, another interview question showed that Department B members ranked their satisfaction with associates higher than Department A members did. How is this apparent paradox explained? For an explanation, we can turn to an excerpt from Department B interview data. The excerpt typifies feelings expressed by other group members.

Friendships in here are more intermediate than close. Possibly that's due to the degree of freedom we have. If we didn't have the freedom then we'd prob-

ably each be seeking allies and there would be a tendency for people to cling to each other.

In effect, Department B members explain their preference for outside as well as inside friendships in three ways. These are (1) the lack of any common antagonist, (2) the tendency to avoid affective friendships with potentially competitive associates, and (3) the desire to build up family friendships rather than work-related individual friendships. Equally important, Department B members apparently had opportunities to build up friendships outside the department in the company. Most Department A members had fewer work-related opportunities for outside interaction. Despite their lower mutual friendship involvement, Department B members tended toward strong participation in outside social activities. Department B members, furthermore, expressed themselves as "highly active" in outside activities significantly more times than Department A members. Department B members also found many more areas of interest in common with each other than Department A members did.

Performance and Satisfaction Patterns

As a matter of record, Company B had a successful history of product innovation and development. Although the vice president of manufacturing mentioned the importance of informality for Research and Development, two other officials related company success to the company-wide informality typified by the president of Company B. However, despite a high emphasis on the values of individual autonomy and development, and a lesser exclusive emphasis on productivity, Department B members apparently produced highly accurate apparatus on schedule. At the same time, during the research, Department B received considerably fewer complaints from either customers or other departments than had Department A. B's performance record and reputation were considerably stronger according to management appraisals, customer complaints, and the feelings of other departments in the company.

The data on the satisfaction of the employees in both departments show that Department B members reported significantly higher satisfaction than Department A. Interview data disclosed that for Department A members, the grass tended to look greener in other departments, but formal policy made it difficult for an individual to transfer. Department B members tended to feel that the existing situation was too good to leave, even though one could. In Department A, 57 percent of the members knew of other jobs they would like equally well or better. In Department B, the figure was 13 percent.

Department A's relatively closed system fostered an emphasis on immediate productivity and practicality, while possibly neglecting the longer range individual needs for growth and development. Ironically, when these individual needs received high consideration by management,

as in Department B, the search for outside opportunities decreased even though the opportunities themselves tended to increase.

Status Patterns

The data for Department A showed that four out of eight subgroups confined themselves to a single status level. By contrast, eight of Department B's nine subgroups encompassed two or more status levels. One of Department B's subgroups included representatives from all four job status levels. Furthermore, in Department B an engineer's or a technician's relative job status did not necessarily relate to his position in the social organization of the department.

In Department B, management emphasized the importance of individual contribution regardless of status position. The stress on job autonomy and mutual influence between status levels supported this value with its corollary that neither job status nor social status should interfere with knowledgeable contribution.

The social status factor of formal education level, however, was something else again. This factor seemed to relate to the behavior, performance, satisfaction, and salary levels of individuals in Department B. This raises a difficult but important question: How did contribution potential relate to formal education level? Were they highly related in Department B, or was management possibly confusing a status indicator with an individual's actual competence? Put another way, was Company B, built upon a value system avoiding stress on social status factors, beginning to shift direction? Was the formal status indicator becoming more important than the actual ability to contribute? The data suggested the beginnings of such a change. Any restlessness among lower education level engineers, however, was balanced by their own awareness that they "were lucky" to be where they were in Company B.

In short, Department B was becoming caught between (1) the democracy and informality of its management traditions and values on the one hand, and (2) the realities of present-day technology on the other. The first stressed individual autonomy and the situation's fitting the individual. The second emphasized technical sophistication and high formal education as a preliminary necessity. In one sense, Company B's dilemma resembled the mid-twentieth century problem of any technically oriented organization.

But in another sense, Company B's problem was different because of its particular organizational traditions and values. Whereas Company A's relatively closed system posed serious conflicts between management controls and scientific integrity, Company B's relatively open system posed stress points between formal science values and *individual* growth and integrity. As science and technical knowledge became more important to Company B, they presented a unique problem for a relatively open

system. Instead of developing reference group conflicts between science and management, as in Company A, Company B management could adopt science's value system at least to the extent that the delicate balance among business values, science values, and individual informality could be maintained. If, however, this balance was upset, the formal education level might unfortunately overshadow actual contribution as management's chief measure of competence. If this should happen, Company B's relatively open system might begin to close.

Uncertainty and Open Systems

It will be recalled that in Department A the level of each key status characteristic tended to reinforce the others; a man knew just where he stood in this department and it was difficult for him to change this position. The situation in Department B differed sharply in that there were no sharply defined reference groups. Furthermore, status factors in Department B were not in line, but incongruent, thus often placing the individual in a position of ambiguity and uncertainty. In such an ambiguous position, he could act as the situation demanded rather than in line with what was expected of a man in his position.

However, it is important to point out that the behavioral patterns observed in Department B occurred at the expense of clear-cut status authority. Both the head of field engineering and Supervisor B encouraged mutual influence, sometimes in spite of the uncertainties and discomforts accompanying it. Thus, in order to supervise a relatively open system, Supervisor B and his superiors relinquished much of the status authority, downward influence, and social distance they might have claimed. While some part of their status as authority figures decreased, the pressures upon them as involved individuals increased. In a sense, they needed both perspective and involvement in the day-to-day problems of others. But the ambiguity of this position created interpersonal relationships which inevitably became more open, less status centered, and more helpful to each person involved.

Furthermore, within this situation, uncertainty became legitimate. Downward influence no longer determined what was practical since influence became a mutual process. However, the behavioral dynamics within Department B required uncertainty and some frustration on the part of Supervisor B. His world was no industrial utopia but rather a series of technical, personal, and procedural problems. Each was challenging, but each seemed solvable within a relatively open system.

IMPLICATIONS FOR MANAGEMENT

Very briefly, the findings question the usefulness of management orientations which stress profits, productivity, and practicality to the

exclusion of other values. Organizational values, according to the findings of this study, may be overstressed and self-defeating in technical groups. Management representatives defend and define practicality in the hopes of swaying scientist-engineers toward understanding the competitive realities of life. But they do more. Management is in a position to impose its own practicality on groups of scientist-engineers in industry, and these organizational values raise problems. The findings suggest that re-examination in terms of what is and what is not "practical" would be useful within organizational contexts. This suggestion comes cautiously, for the notion of practicality is management's own pride and province.

From this point of view, industry's practicality concept can often be described in terms of some related components. These components include formal policies and procedures, careful organizational structuring, formalized supervisory responsibilities, and highly defined employee obligations. In this sense "practical" management stresses the importance of formal policies and procedures which define controls and boundaries so that everyone is relatively certain of who does what, when, how, and where.

These policies, procedures, and structures require the practical manager to seek greater certainty and clarification of the organization around him. In some respects this world of controlled certainty is useful, but, as in Department A, it also tends to foster a related world of unanticipated rigidities. In the light of these negative consequences, and against the background of Departments A and B, the following tentative conclusions become pertinent since they also are concerned with notions of practicality:

1. Conditions of high status congruence in technical groups may offset a desired output of ideas, improvements, and product development.
2. Management may defeat its own goals by stressing organizational values to groups whose backgrounds encourage their seeking other, more distant reference groups.
3. Reward systems in relatively closed systems may artificially reward an individual for his social status rather than for his actual work contributions.
4. Organizational structures that formally restrict an individual to a relatively narrow subgroup activity may generate subgroup competition and block individual development.
5. "Span of control" concepts in technical groups may introduce supervisory control limits at the expense of subordinate autonomy and development.
6. Highly structured formal policies and procedures run the risk of confusing ends with means, and structure with efficient informality.
7. Competitive conditions within technical departments may be avoided by structures and procedures which help establish departmental goals rather than subgroup goals.
8. Formal follow-up procedures that stress productivity may do so at the expense of a superior's capacity to help subordinates achieve that productivity.
9. Relatively closed organizational systems tend to encourage influences, controls, and interactions that separate management logics from subordinate needs and contributions.
10. Management probably can hold a supervisor accountable for his group's

productivity and satisfaction only if it explicitly takes account of the relationship between the supervisor and his own superior.

Management might well experiment in order to move toward relatively open organizational systems. It could design and carry out its own research in organizational behavior as one way of systematically discovering what happens when greater autonomy is encouraged and when a relatively closed system begins to open. Likewise, management can tentatively explore procedures that help build effective mutual infleuence relationships between status levels. Finally, interaction can easily be encouraged so as to extend beyond the confines of job requirements. These experimental steps can lead to both excitement and uncertainty. They can lead to relaxed relationships with scientists and engineers, or to rationalizations for a return to a closed system. One thing is clear: the management willing to experiment and openly evaluate its experiences in these uncertain directions runs the best chance of succeeding.

Chapter 9

LEADERSHIP STYLE AS A VARIABLE IN RESEARCH ADMINISTRATION*

HOWARD BAUMGARTEL†

IN AN earlier issue of this *Quarterly*, D. C. Pelz summarized briefly the findings of this author's investigation into the relationship between leadership style and the motivations and attitudes of laboratory scientists in a large government medical research organization.[1] This paper reports more fully the methodology and detailed findings of one specific phase of the analysis summarized by Pelz, namely, the identification of three styles of laboratory leadership and their effect on those laboratories. Current widespread interest in leadership calls for further clarification of empirical research findings in this field. Description of this particular study can throw some light on its significance for human-relations training and administrative practice.[2]

WHY STUDY LEADERSHIP STYLE?

Many American administrators hold simultaneously two somewhat contradictory beliefs about people in organizations. One is that the behavior of members of an organization is determined primarily by stable personality characteristics, or "traits." The second is that effective organization is created by certain outstanding individuals to whom are attributed indefinable and almost superhuman skills and abilities—they believe, in other words, that good leadership cannot be understood.

Social science theory and research during the past quarter of a century has questioned both of these beliefs. Such overindividualistic views deny the determining effects of situational and social factors in behavior. In

* Reprinted with permission from *Administrative Science Quarterly*, December, 1957, pp. 344–60.

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[1] D. C. Pelz, "Some Social Factors Related to Performance in a Research Organization," *Administrative Science Quarterly*, Vol. 1 (1956), pp. 310–25.

[2] The study was financed by the National Institutes of Health, U.S. Public Health Service, and U.S. Department of Health, Education, and Welfare. D. C. Pelz was the study director. Pelz, R. Davis, and G. Mellinger developed the questionnaire and collected the data on which this particular analysis was based. The author of this article joined the study at a later date.

administration, they lead to a preoccupation with selection and placement as methods for improving and changing organizations.[3] These beliefs persist, however, with a tenacity which leads to the suspicion that they are functional in organizational life although partially mythical in character. Why is this so? Perhaps administrators' egos are reluctant to recognize situational determinism, and it is often convenient to blame organizational problems on the personality "traits" of subordinates.

We live at a point in history when the validity of such beliefs about the nature of man's behavior in work organizations is no longer a matter of academic debate. With modern research methods it is now possible to test beliefs against empirical data. The qualities of effective leadership have been studied in factory and office work groups and in experimentally created laboratory groups. The present study aimed at extending the generality of these findings to the setting of a highly complex, large-scale research organization composed of sophisticated professional personnel.

The study had two specific objectives. One was to explore the applicability of the concept of participatory leadership and its counterparts, the directive and laissez-faire types, in this new setting. The leadership typology theory had its origins in the well-known Kurt Lewin, R. Lippitt, and R. H. White study of leadership climate.[4] Besides testing these leadership styles, the present study aimed at clarifying their operational characteristics by asking what their basic dimensions are and how they are measured.

The second objective was to focus on second- and third-level administrative personnel rather than on top-level or face-to-face relationships. In a four-year longitudinal study of morale measures conducted by the Survey Research Center in The Detroit Edison Company,[5] personnel changes at the department-head level were found to have had dramatic effects on the attitudes and satisfactions of nonsupervisory employees. This Detroit Edison study and the relatively few studies dealing with the effectiveness of human-relations training programs have revealed the need to study second-level leadership practices. A number of such studies indicate that the attitudes and behavior of second-line supervisors are important conditioning variables determining the effectiveness of the training of foremen.[6]

The purpose of the present study, then, was to test the applicability of

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[3] Carefully validated personality tests can, of course, improve selection-and-placement procedures.

[4] R. Lippitt and R. H. White, "An Experimental Study of Leadership and Group Life," in T. M. Newcomb and E. L. Hartley (eds.), *Readings in Social Psychology* (New York, 1947).

[5] Unpublished data.

[6] Floyd C. Mann, "Studying and Creating Change: A Means to Understanding Social Organization," in Conrad M. Arensberg *et al.* (eds.), *Research in Industrial Human Relations: A Critical Appraisal* (Industrial Relations Research Association, Publication No. 17) (New York, 1957), pp. 146–67.

the participatory, laissez-faire, directive leadership typology to the behavior of second-level research administrators and to test certain predictions about the relationship of leadership style to the motivations and attitudes of subordinates, in this case the laboratory scientists.

THE RESEARCH SETTING AND METHOD

The research organization itself is composed of several major research institutes, each specializing in a particular disease category.[7] A central administrative and service structure supports the work of the organization as a whole. Each major institute is in turn composed of several research laboratories which are specialized along the lines of the scientific disciplines. There are 20 of these research laboratories containing in all 330 scientists having M.D. and Ph.D. degrees. (Data on nonprofessional personnel were not included in this study.) Twenty of these professional researchers are the directors of the 20 laboratories, 68 are intermediate research supervisors, and 242 have no supervisory responsibilities for other professionals.

The twenty laboratory directors and the scientific personnel in their laboratories were chosen as the basic units of analysis for this study of leadership style. With minor exceptions, each of the twenty laboratories contain at least two levels of hierarchical supervision. The research questionnaire was designed to provide specific information about the behavior of the laboratory director as seen by at least two levels of subordinate scientists. These questionnaires were filled out by all employees at all levels, both professional and nonprofessional. They covered areas of perception, satisfactions, attitudes, values, and so on.

The laboratory units differed sufficiently in size and composition, however, so that special statistical procedures were required to take these differences into account in testing the predicted relationships between leadership style and the attitudes and motivations revealed in the responses of the laboratory scientists.

The twenty laboratories varied in size from six to thirty-four professionals employed, with a mean of 16.5. In addition, the laboratories differed in (1) the average length of service of personnel (closely correlated with differences in average age), (2) the average grade or rank, hence income, of the scientists, and (3) the number of hierarchical levels within the laboratory. Prior investigations of the data showed that these variables are related to some of the attitudinal measures used in this study. Other differences in the composition of the laboratories, such as the relative proportion of M.D. and Ph.D. scientists, appeared to have little relevance and hence were disregarded in the analysis.

The twenty laboratory directors were generally older than their

[7] For further information on this study see *Interpersonal Factors in Research* (Ann Arbor, Mich.: Institute for Social Research, 1954), Part I.

subordinates. They varied in age from thirty-six to sixty-one, with an average age of forty-eight. They also differed considerably in their length of service with the research organization and in their length of service as laboratory directors. No attempt was made to relate the analysis to these background differences between the laboratory directors.

The laboratory director has a difficult and complex job. He functions both as a director of research and as an administrator of various policy, service, and liaison functions. He is assisted in purely administrative matters by the nonprofessional staff of the organization. One of the director's major leadership functions is his activity in the general process of molding research programs which are suitable to the demands of various external groups and, at the same time, are proper in terms of the development of scientific knowledge and the research interests of the working scientists. In view of the importance of this function, the questionnaire focused attention on the relationship of the laboratory directors with their subordinates in deciding upon new projects, assigning people to tasks, allocating funds and equipment, and interpreting research results.

All of the data used in this study were derived from the questionnaire responses of the scientists in the twenty laboratories. The items dealing with the decision-making processes of the laboratory directors and other related items provided the basic information for determining the leadership style of each particular laboratory director. Measures for assessing the "effects" of the various leadership styles were developed from other kinds of questionnaire information. Questions which appeared to measure the strength of the scientists' research orientation and their sense of progress toward research goals were used for this purpose because of their close logical relationship to the concept of organizational effectiveness. In addition, questionnaire items dealing with attitudes toward, and evaluations of, the laboratory directors (as opposed to behavioral description items) were used as criterion measures. These variables are discussed more fully below.

The questionnaire data were analyzed as follows:

1. A mean score was computed for each laboratory on each relevant questionnaire item. Likert-type responses were designed to permit group comparison

2. The laboratory mean scores on each item were converted into rank-order scores from 1 to 20, with 1 representing the first or "best" score. These rank-order scores then became the basic measures for the laboratory analysis.

3. The analysis itself consisted of establishing the relationships among the rank orderings of the laboratories on one measure with their rank orderings on another. Rank-order correlations or the comparison of the mean rankings of various groupings of laboratories were used to establish the empirical findings.

4. Tests of statistical significance were used throughout to reduce the possibility of attaching meaning to chance relationships.

The first step in the analysis was to establish operational measures by which the leadership style of the laboratory directors could be established.

ESTABLISHING THE LEADERSHIP STYLE OF THE LABORATORY DIRECTORS

The scores on the questionnaire items dealing with the behavior of the laboratory director revealed marked differences between the laboratories. The research problem then presented itself as that of discovering the basic dimensions of these differences which would permit the integration of all the item differences into one consistent scheme. Low and insignificant correlations between some measures were noted; for example, laboratory rankings on "frequency of contact" with the laboratory director were unrelated to rankings of the perceived "influence" of the director's decisions on the laboratory scientists. This indicated that there was more than one dimension to the pattern of relationship between the directors and the scientists in their laboratories.

Further exploratory analysis of the laboratory rankings on the leadership-behavior items revealed two major independent dimensions of leader behavior. One of these dimensions was labeled *degree of involvement* and was measured by an index consisting of the combined scores from two correlated questions: "frequency of contact" with the director and the amount of "influence" on the director reported by the laboratory scientists. The other was called *locus of decision* and was measured by an index formed by combining the scores on two other correlated items: the proportion of laboratory scientists who reported making decisions "on their own" and the perceived "influence" of the director's decisions and activities on the scientists in his laboratory. A consolidation of these two dimensions of leadership interaction provided the basic framework for characterizing the leadership style of any particular laboratory director (Fig. 1).

The model in Figure 1 provided for a three-way classification of leadership style which was consistent with the original intent to explore the utility of the Lewin, Lippitt, and White triad. The terms "laissez-faire," "participatory," and "directive" were chosen to distinguish this model from the autocratic, democratic, laissez-faire model in order to avoid some of the value connotations which have come to be associated with the words "autocratic" and "democratic." All but two of the twenty laboratories fell within the scheme. The directors of these deviant laboratories were characterized by so much inconsistency in the data that they were eliminated from the study as representing "impure" types. Hence six laboratory directors were identified as laissez-faire in style,

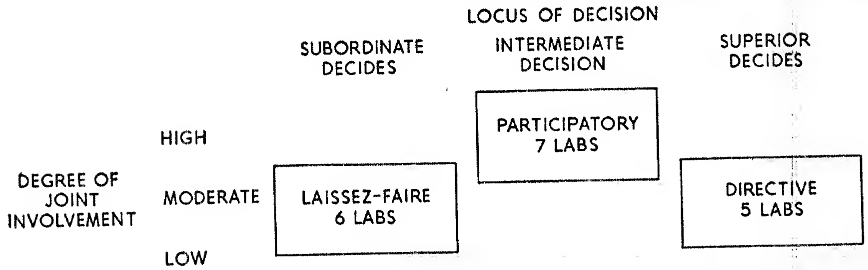

FIG. 1. Model for characterizing the leadership style of the laboratory directors.

seven as participatory, and five as directive. A summary of the leadership characteristics in the three different groups of laboratories is presented below:

In the six *laissez-faire* laboratories:

Director's decisions have little influence.
Many subordinates feel that they are on their own.
Few report joint discussion and decision.
Infrequent contact with director is reported.
Subordinates have little influence on director.
Very few report that director decides things.

In the seven *participatory* laboratories:

Director's decisions have moderate influence.
Some subordinates feel that they are on their own.
Many report joint discussion and decision.
Most frequent contact with director is reported.
Subordinates have much influence on director.
Few report that the director decides things.

In the five *directive* laboratories:

Director's decisions have much influence.
Few subordinates feel that they are on their own.
Some joint discussion and decision occur.
Frequent contact with the director is reported.
Subordinates have little influence on the director.
Many report that the director decides things.[8]

Some corroboration of this characterization of leadership styles was obtained from an examination of the questionnaires filled out by the directors themselves. For example, the participatory directors indicated that they spent more time in professional work and worked longer hours than were reported by their directive and laissez-faire counterparts. This is consistent with the presence of a high level of interaction and involvement with subordinates among the participatory laboratory directors.

[8] All of these differences are statistically significant at the .05 or .10 level except one which approaches the .10 level.

Furthermore, their self-reported decision practices tended to conform to the three-way classification developed on the basis of subordinate perceptions.

Several complicating factors should be noted, however. The participatory directors tended (the results were not statistically significant) to have higher scientific performance ratings and to place a higher value on science goals than did the other laboratory directors.[9] In another analysis of the discrepancy between the director's actual behavior and what his subordinates would prefer him to do, small differences tended to be present in the laissez-faire and participatory laboratories, while larger differences were observed in the directive laboratories (again not statistically significant). The small number of cases in this study made it impossible adequately to control the possible contaminating effects of these other differences between laboratory directors.

To summarize, the actual distribution of laboratory scores on the leader-behavior items made it possible to classify the laboratory directors by this three-way typology in a manner which was both consistent with other leadership studies and internally consistent with a variety of data from the study itself. It should be emphasized that a two-dimensional scheme was used, employing both the amount of involvement and interaction and the locus of decision. With the leadership styles of the laboratory directors thus identified, it became possible to test for relationships between leadership style and other factors in the situation.

TESTING THE RESEARCH PREDICTIONS

The study predicted that the participatory leadership style, in comparison with both the laissez-faire and the directive styles, would be associated with higher levels of motivation toward organizational goals (research values), a higher sense of progress toward these goals, and more favorable attitudes toward the leader. The logic for these predictions rests on both the sociology and the psychology of participation. Where a subordinate shares in the decision process, he can become more committed to decisions and can more fully internalize the goals of the organization of which he is a part.

Directive leadership, it is thought, leads to overdependence on the leader and hence to lower levels of internalized motivation toward the objectives of the organization. Furthermore, directive leadership often creates suppressed resentments and hostilities which impair performance and diminish satisfaction.

The limitations of laissez-faire leadership, on the other hand, lie in the inherent problems of organization life. In the laissez-faire situation sub-

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[9] See R. C. Davis, "Factors Related to Scientific Research Performance," in *Interpersonal Factors in Research,* Part I.

ordinates have no protector or provider on whom to rely. The potential contributions of the leader to shared problem solving are lost. Information needed in the decision process is often not made available. Even a research organization, where autonomy is highly valued, needs performance of certain essential leadership functions for effective operation.

Tables 1 and 2 show the results of the analysis of the relationship

Table 1

A COMPARISON OF THE MEAN RANKING OF LABORATORIES UNDER DIRECTIVE AND PARTICIPATORY LEADERSHIP STYLES

Questionnaire Item	*Mean Rank of Laboratories* Directive	Participatory	*Pos. or Neg. Diffs. & Stat. Sig.*
Research orientation (importance)			
Use of present abilities	13.0	9.9	Pos.
Freedom for originality	12.0	9.1	Pos.
Contributing to basic science	9.4	12.3	Neg.
Sense of progress (extent job provided)			
Use of present abilities	15.3	6.6	Pos.*
Freedom for originality	17.7	8.6	Pos.*
Contributing to basic science	17.0	7.7	Pos.*
Attitudes toward the director			
Enjoyment of contacts	12.5	10.1	Pos.
Confidence in his motives	13.6	11.1	Pos.
His qualifications to help in professional area	11.6	8.1	Pos.
His general helpfulness	10.8	10.3	Pos.
His professional leadership and stimulation (if any)	14.7	9.5	Pos.
His accuracy in evaluating work (if occurs)	14.5	8.9	Pos.†
His familiarity with work in laboratory	10.2	6.7	Pos.
Over-all satisfaction			
Quality of leadership in research organization as a whole	15.9	5.6	Pos.*

* Statistically significant difference in mean ranks, .05 level, one-tailed test.
† Statistically significant difference in mean ranks, .10 level, one-tailed test.

between leadership style and the rank ordering of laboratories on the attitudinal measures. Table 1 compares the directive and participatory leadership styles, while Table 2 compares the laissez-faire and participatory styles. *Research orientation* was measured by scores on three correlated questionnaire items dealing with the importance attached to use of present abilities, freedom for originality, and making a contribution to basic science.[10] *Sense of progress* toward research goals was measured by

[10] Two independent investigations have shown that scientists with higher performance ratings attach more importance to these values than do scientists with lower ratings (see D. C. Pelz, *op. cit.*, pp. 312–13).

scores indicating the extent to which laboratory scientists felt that their job situation actually provided for these same three research values. *Attitudes toward the director* were measured by laboratory scores on the seven questionnaire items which referred specifically to the laboratory director. One other leadership item referring to the *over-all satisfaction* with the quality of leadership in the organization as a whole was also included in the analysis.

Table 1 compares the mean ranks of the five laboratories under directive leadership and the seven laboratories with participatory leadership. In thirteen out of the fourteen laboratory comparisons the predictions are supported. Only in the case of scores on the importance attached to contribution to basic research is the direction reversed.[11] The greatest differences in attitudes appear in the items dealing with sense of progress or achievement. Evidently, scientists in the participatory laboratories feel that their jobs provide for use of abilities, freedom for originality, and making a contribution to basic science to a much greater extent than do scientists in directive laboratories. Also there is a marked difference in the over-all satisfaction with the general quality of leadership in the laboratories under participatory leadership. It would appear that over-all satisfaction with the administration of the whole research organization is closely associated with the behavior of the laboratory directors.

As was mentioned earlier, differences in the number and composition of the personnel of the laboratories required special statistical procedures in evaluating the relationships with leadership style. Several significant and interesting findings derived from this analysis (not shown in the table). In comparing the responses of longer- and shorter-service scientists under the two leadership styles, it was found that younger scientists under participatory directors indicate significantly higher motivation to contribute to science than do younger scientists under directive leadership. This is in spite of the general trend in the other direction, the one negative finding noted above.

When the element of laboratory size is controlled, the beneficial effects of participatory leadership show up more noticeably in the smaller laboratories. On the other hand, in items dealing with commitment to research values, participatory leadership results in more importance being attached to research goals in the larger laboratories. In comparing separately the results for scientists having higher grade or rank with those of lower grade or rank, it appears that, in general, the predicted relationship of leadership style with attitudes is greater among higher-graded scientists

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[11] Actually a very high proportion of the scientists in all laboratories attached the utmost importance to contributing to basic science, so that the variation on this item was not great. It has been suggested, however, that high levels of deprivation of important needs can act to increase need strength (see N. Morse, F. Mann, and R. L. Kahn, "The Meaning of Morale," paper delivered at the Conference on Theory of Organization, Princeton University, 1952).

than among lower-graded scientists. Finally, there is a tendency for the positive differences in attitudes of scientists under participative, as compared with directive, leadership to be greater among those who report directly to the laboratory chief. Differences in leadership style have little effect on second- and third-level scientists.

In general, then, a comparison of the laboratory mean ranks under directive and participatory leadership indicates that scientists under participatory leadership (1) are somewhat more motivated toward research orientation, (2) have a markedly higher sense of achievement in research, and (3) hold generally more favorable attitudes toward their director. These relationships between leadership style and attitudes cannot be accounted for by other differences between the laboratories, such as size, average length of service, and so on. Furthermore, the differences between laboratory groups show up stronger in smaller laboratories, among higher-graded scientists, and among scientists who report directly to the laboratory chief.

Several implications for the selection and training of research administrators follow. Not just research proficiency and knowledge of the organization but also patterns of interpersonal work relations need to be taken into account when new appointments to leadership positions are made. In training activities, research administrators who are inclined toward directive methods should be encouraged to develop skills in sharing decisions with subordinates. Directive leadership should be particularly discouraged in small research groups consisting of relatively high-level and experienced scientists. To the extent that the low sense of personal achievement in laboratories having directive leadership reduces attachment to the laboratory as a place to work, one might expect a higher turnover of professional personnel in such an environment.

Table 2 compares laissez-faire and participatory leadership. In this comparison ten out of fourteen differences favor the predicted relationships between leadership style and attitudinal measures of laboratory personnel and three oppose the predictions. Those differences showing the strongest support for the predicted benefits of participatory leadership are the following: scientists under participatory leaders attach significantly more importance to having freedom for originality; they feel that their job situations provide for fuller use of their abilities; they feel that their directors are more qualified to give professional help and are more familiar with the work of the laboratories. Also, as in the comparison between directive and participatory leadership, scientists under participatory leaders show much higher satisfaction with the general quality of leadership in the whole organization than do those under laissez-faire leaders.

The analysis employed on the effects of factors other than differences in the leadership style (not shown in table) generally do not alter these findings. The differences in favor of the predictions are generally greater

for short-service scientists than for long-service scientists. Longer-service scientists under participatory leaders, however, indicate that they have much more freedom for originality than those under laissez-faire directors. There is some tendency for laissez-faire leadership to have more negative effects among the larger laboratories than among the smaller laboratories. When comparing separately the responses of higher- and lower-graded scientists under the two types of leadership, it was found that higher-

Table 2

A Comparison of the Mean Ranking of Laboratories under Laissez-Faire and Participatory Leadership Styles

Questionnaire Item:	*Mean Rank of Laboratories* *Laissez-Faire*	*Participatory*	*Pos. or Neg. Diffs. & Stat. Sig.*
Research orientation (importance)			
Use of present abilities	11.1	9.9	Pos.
Freedom for originality	13.0	9.1	Pos.*
Contributing to basic science	11.7	12.3	Neg.
Sense of progress (extent job provided)			
Use of present abilities	12.4	6.6	Pos.*
Freedom for originality	9.0	8.6	Pos.
Contributing to basic science	9.8	7.7	Pos.
Attitudes toward the director			
Enjoyment of contacts	11.0	10.1	Pos.
Confidence in his motives	8.4	11.1	Neg.
His qualifications to help in professional area	14.3	8.1	Pos.*
His general helpfulness	10.9	10.3	Pos.
His professional leadership and stimulation (if any)	9.3	9.5	Neg.
His accuracy in evaluating work (if occurs)	8.8	8.9	—
His familiarity with work in laboratory	15.4	6.7	Pos.*
Over-all satisfaction			
Quality of leadership in research organization as a whole	12.8	5.6	Pos.*

* Statistically significant difference in mean ranks, .05 level, one-tailed test.

grade scientists again indicated strongly that participatory leadership was associated with more favorable attitudes. The effects of the hierarchical distance of the scientists from the laboratory director were somewhat ambiguous, although there was a slight tendency, as in the comparison with directive leadership, for the relationships to be strongest among those reporting directly to him.

In comparing laissez-faire and participatory leadership in laboratories, then, it appears that the relationships with attitudinal measures are less pronounced than in the comparison between directive and participatory leadership. The over-all results, however, indicate that the laissez-faire

leadership style is associated with lower commitment to research orientation, less sense of progress toward research goals, and several less favorable evaluations of the laboratory director.

Because of the consistent pressures toward laissez-faire leadership in research administration, it is of interest to find that this comparison of participatory and laissez-faire styles suggests some important considerations. Since scientific values demand a high degree of personal autonomy, working scientists often indicate a desire for laissez-faire leadership. Furthermore, many research directors prefer working at their own professional research problems to exercising their leadership functions. The findings of this study suggest, however, that laissez-faire leadership is not the most appropriate for research laboratories in an organizational setting. Interestingly enough, scientists under participatory leadership actually feel a greater sense of freedom for originality than those under laissez-faire leaders. Research administrators might well be encouraged to avoid laissez-faire leadership and to enter into a more active role with their subordinates. In the selection or training of directors for large laboratories or for working with younger scientists, particular care apparently should be taken to avoid the laissez-faire pattern.

Figure 2 presents, in summary form, comparisons of the three different leadership styles. As can be seen, directive leadership is associated with the lowest mean ranks on the combined measures, while participatory leadership is associated with the highest. The two attitude areas showing the strongest relationship with differences in leadership style are overall satisfaction with leadership and sense of progress in achieving science goals. Measures of the importance attached to research orientation and of the attitudes toward the director himself have less association with leadership style.

CONCLUSION

Eighteen of twenty laboratory directors in a large government medical-research organization were identified as employing three different leadership styles—participatory, laissez-faire, and directive. Participatory leadership was characterized by a high degree of interaction and involvement with subordinates and joint decision-making practices. Laissez-faire leadership was characterized by a low degree of interaction and involvement and high autonomy in subordinate decision making. Directive leadership was characterized by a moderate degree of interaction and involvement, with decisions being made most often by the laboratory director. As predicted, participatory leadership was found to be associated with the highest scores on a number of different measures of the motivations and attitudes of the scientists in the eighteen laboratories.

The results of this study suggest that high-level professional personnel do respond to situational factors in organization. The leadership climate

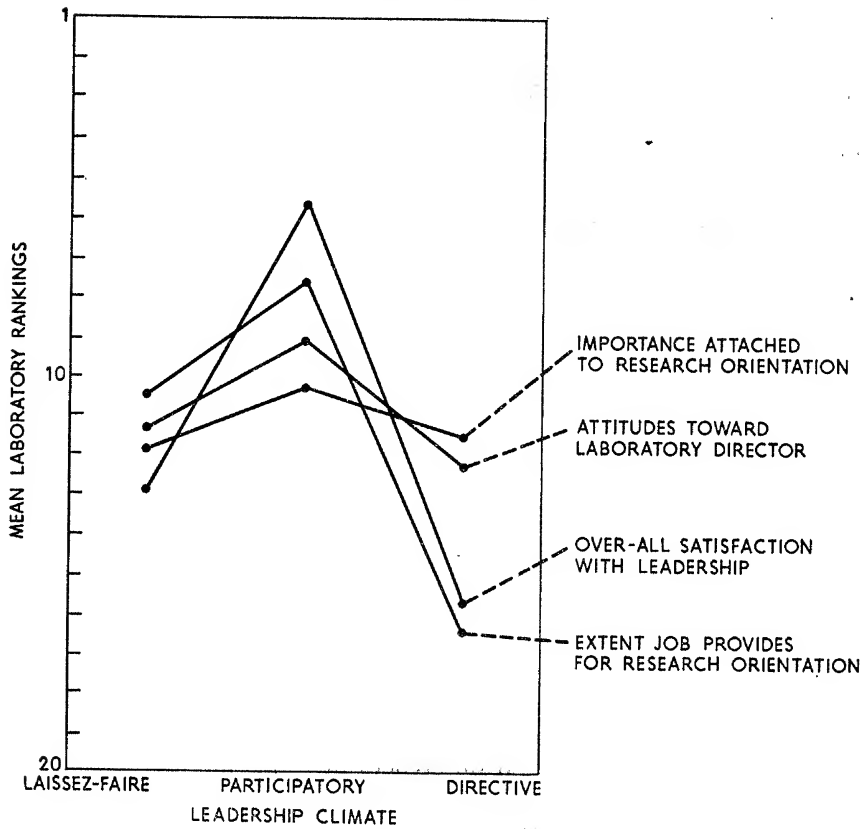

FIG. 2. Summary of relationships between leadership style of the laboratory directors and laboratory mean ranks by attitude area.

within which the scientist works is thus an important variable in determining his motivations and attitudes. Furthermore, the analysis of leadership style itself indicates that effective leadership is not beyond measurement but rather can be identified and built into training and selection programs. The specific results of the study support the validity of the concept of shared leadership as a realistic way of achieving more effective performance and more personal satisfaction.

Chapter 10

SOME ORGANIZATIONAL FACTORS AFFECTING CREATIVITY*

NORMAN KAPLAN†

IN RECENT years there has been a tremendous increase in the concern with creativeness. With the exception of studies such as those by Galton,[1] it is only in the last decade or so that social scientists and others have become interested in studying the creativeness of relatively large segments of the population. The early work of a decade ago was concerned chiefly with the creativeness of the artist, the poet, and the writer, but the expansion of scientific research activities since World War II very quickly resulted in a strong focus on the creativeness of the scientist. Most of this work has been concerned with the identification of creative persons and with a search for predictors of creative talent. This research has proceeded on two levels. First, on the immediate and somewhat practical level, there has been considerable interest in devising "tests" and other criteria for use in hiring research personnel. Second, on a slightly longer-range level, there has been a tremendous concentration of effort on the "early" identification of creative talent in the grade schools and high schools.

Psychologists have been in the forefront of this renewed interest in creativity. By and large, they have entered this field with the assumptions and the tools that have been used in other areas of psychology. Those psychologists wedded to testing techniques have been busily engaged in devising tests for creativity—many of these are modifications of existing tests or have been developed from them. Psychologists with a clinical background have been energetically pursuing studies involving the Rorschach, the T.A.T., as well as a host of other projective tests of personality. Prediction studies have used the logic which has become so familiar in many other areas, such as the prediction of marital success, in which a number of individuals who are judged to be "successful" are

* Reprinted with permission from *IRE Transactions on Engineering Management*, March, 1960, pp. 24–30.

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[1] F. Galton, *English Men of Science* (London: Macmillan & Co., Ltd., 1874); *Hereditary Genius* (2d ed.; London: Macmillan & Co., Ltd., 1892).

studied to determine those factors which may be related to this success. Then various "test" groups are put through batteries of tests to see the extent to which they possess the same characteristics. If they do possess these characteristics with fairly high frequency it is then assumed that they are more likely to become successful. Thus, Flanagan[2] and others have studied the behavior of many scientists and the conduct of their research and have selected certain factors which seem to "go with" scientists judged to be creative. Then they have incorporated these factors into tests to be administered to prospective scientists. General theoretical discussions of creativeness and careful conceptual analysis have on the whole been absent from this psychological literature with the noteworthy exception of several papers by Stein,[3] Roe,[4] and a handful of others.

Creativeness or creativity, as it is more widely called, has become largely a function of the varying measurement techniques used in the specific studies. Tests have been made reliable and "valid" by sophisticated statistical measures but rarely is there any comparability between studies because so little is said about the concept of creativeness itself. Sometimes the discussions would lead one to believe that the creativeness being measured is that of an Einstein or a Newton. At other times it seems to be of a somewhat lower order, typically found in fairly large proportions of the population. We are beginning to have many measures but we do not really know what we are measuring.

Certainly there has been less concern with and very much less empirical study of the organizational or the environmental factors which promote or inhibit the creativity of the scientist in the research laboratory. The broader type of environmental factor has received little more than passing attention—especially such factors as the importance of the school system, the home, and the community. Yet the society as a whole, and particularly its dominant cultural values, may be very important factors in encouraging or discouraging the growth of creative individuals. Clearly, the emphasis in our own society on technological change and planned obsolescence has placed a high premium on certain kinds of innovation sometimes labeled creativity, such as new names for products, new labels, new packaging devices, or a slightly different design. It is this emphasis which is probably responsible in part for the growth of interest in research on "creativity."

The main concern of this paper is with the factors considered important by those directing creative scientific research. However, a plea will also be made from time to time for more precise and more systematic conceptions

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[2] J. C. Flanagan, *et al., Critical Requirements for Research Personnel* (Pittsburgh, Pa.: American Institute for Research, 1949).

[3] M. I. Stein, *et al., "A Case Study of a Scientist,"* in H. Burton and R. E. Harris (eds.), *Case Histories in Clinical and Abnormal Psychology* (Clinical Studies of Personality, Vol. 2) (New York: Harper & Bros., 1955).

M. I. Stein, "Creativity and Culture," *J. Psychol.*, 36: 311–22 (1953).

[4] A. Roe, *The Making of a Scientist* (New York: Dodd, Mead, & Co., 1953).

of creativeness and for increased attention to the larger environmental factors which may promote or inhibit creativity in the society at large.

The factors to be discussed here are derived from interviews conducted in some twenty research organizations with directors of research, administrators, and scientists at various levels in the research organization. These men were asked how they defined creativity in their own laboratory, how they recognized it, how they rewarded it, and how they recognized its potential in prospective employees. They were also asked to discuss the environmental conditions in their own organization which they felt promoted or inhibited the kind of creativity in which they were interested. It is on information from these interviews that this discussion is based. It should be recognized at the outset that these are preliminary data gathered in a pilot study, that they are not very precise nor are they quantitative. It should also be recognized that other social factors, not considered in this study, are also very important in their effects on creativeness. The individuals being studied have spent many years in educational institutions and perhaps in other research organizations and have had experiences which may have encouraged them to use their full genetic potential of creativeness, and to develop this genetic potential over many years. The fact that these scientists have chosen to work in particular kinds of research organizations, on particular types of problems, is yet another limiting factor in any broad analysis of creativity. Nevertheless, it may be instructive to inquire into the factors considered important within the research organization itself for working with whatever creative potential is "given."

It should be noted at the outset that while there is substantial agreement among these research directors on the general class of factors we shall be discussing, there is considerably less agreement on the direction in which the factors work or on their effects. We shall want to suggest hypotheses about them which might be tested more precisely in further studies.

RECEPTIVITY TO NEW IDEAS

Among the factors most often mentioned as being important in stimulating creativity within the laboratory was a positive and enthusiastic reception to new ideas.

Once said, this is, of course, obvious. If the term "creativity" means anything at all it suggests new ideas. Receptivity to new ideas is frequently mentioned as an important prerequisite for an individual to be creative. At the organizational level this may become somewhat more complicated. Individuals within the organization with varying degrees of receptiveness to new ideas may mean that the over-all institutional receptiveness to new ideas may vary according to the nature of the particular project, the immediate supervisor, the importance of the project to the organization, and many other such factors. Thus, something which is likely to be taken

for granted from the point of view of the individual becomes less obvious as we look at it in the context of an organization.

Everyone interviewed agreed on the importance of this particular factor but further inquiry showed that there is a fairly wide range in conceptions of what a new idea is and how it is received. In those research organizations (whether in industry, government, or even universities) committed to specific research projects and sometimes even larger-scale research programs, the new idea is "wonderful" if it is "on the beam." That is, if the new idea pushes the project ahead, if it gets it nearer to completion, if its does so more expeditiously, then it is a wonderful new idea. If, on the other hand, the new idea is likely to "sidetrack" the project, to lead it in a different direction and lead to very different kinds of answers from those expected and in quite different areas, then it is not so wonderful. The new idea must be *relevant* to be greeted with enthusiasm. Given the nature of scientific research, what is considered relevant at any given point is frequently an educated guess at best. However, many organizations appear to be less than enthusiastic about new ideas where it can be guessed that the relevance is not very close or direct.

Even when the new idea is not considered completely relevant to the current project, there is a fair chance that it will still be entertained and considered seriously for instigating new research projects. This is a common occurrence in the history of scientific research and appears to happen fairly frequently even in larger organizations. However, the procedures necessary for the establishment of new projects, as well as the limitations of budget and personnel, may delay the instigation of a new project for a considerable period of time. Here again there is fairly wide variation among laboratories. Some are fairly flexible with respect to budget, personnel, and the entertainment of new projects, and can get them started fairly quickly. Others must wait until the next budget year or for special allocations of funds, or must proceed through a relatively cumbersome reviewing procedure before the new idea can be translated into a research project. But even a new project may be a source of considerable discussion because it may lead to results which are not considered part of the over-all program of the research organization. For instance, if a research project which is geared to yield a product turns up a result which is scientifically interesting and which could lead to another product, the idea for this different product may be scrapped completely because it is not relevant to the goals of the particular organization. Thus, relevance itself takes on a number of dimensions. It can be limited merely to the on-going set of projects, it can be interpreted in a much broader framework in terms of the over-all long-range program of the organization, or it can be viewed in an unrestricted fashion.

Another word for relevance is *practicality*. If the idea is not viewed as being practical then it may not be received too enthusiastically. As one industrial research director put it, "In industry some attention has to be

paid to the practicality of creative work. But that doesn't mean that we just want a plodding lab. . . ." If the scientist in such an organization has come up with a number of such ideas which were not viewed as "practical" in the past, is it very likely that he will venture to suggest similar ideas in the future? Is it not more likely that he will become somewhat discouraged from suggesting any new ideas, or alternatively, that he will restructure his new ideas along a more practical line (or change jobs)? In any case, it is clear that the notion of what is relevant will influence the kind of creativity that may be present or is encouraged in a particular research organization.

Another obvious facet of this problem is simply the way in which the idea is received. A new idea can be greeted with a great deal of enthusiasm—"that's a wonderful idea, Joe"—and nothing further is done about it. It can be taken seriously and action can be taken to begin a new project or to change the direction of the present project or implement it in some other fashion. Yet another way in which the new idea can be greeted is a negative reaction such as, "No, that's taking us off the beam; that's sidetracking us; let's not play around with any new ideas until we finish this."

Unlike the pencil and paper tests of creativity which encourage the subject to think of as many new ideas as he possibly can, the situation in the laboratory is far more complex. We must take into account not only the way in which the idea is received and the reactions of the various people in the organization to it, but also the action that is taken after the idea is put forth. Thus, new ideas may be stifled at the supervisory level, at the next level up, or by the organization as a whole. In this latter case, the idea may sidetrack the organization from its primary goal. Receptiveness to new ideas is not a matter of personal taste (although that certainly may be a factor). It is a matter of the flexibility of the organization, its goals, its personnel policies, and the availability of funds, personnel, and facilities. Without belaboring the point, it should be obvious that the level of creativeness of any given scientist, as well as the over-all level of creativeness in a laboratory, is related to the structure and social climate of a particular research organization. On the more psychological side, it might be relevant to determine whether people with different propensities for creativeness react differently to persistent discouragement or encouragement. This problem may be related in part to the question of incentives which will be discussed below. We turn now to the second in our series of factors considered important in encouraging creativity.

PRESSURE TO PRODUCE

Now we come to an area simply crying for some real empirical research. In my interviews, two apparently conflicting hypotheses have been offered. The first states that the greater the pressure, the more likely

you are to get something. The second states, "Keep the pressure down, make it a relaxed atmosphere and you'll get much more out of a scientist than if you try to put on the pressure." These are not necessarily diametrically opposed, although they are frequently viewed in this fashion because the terms are not specifically defined. We need to know much more about the nature of the pressure, who exerts it, how it is exerted, as well as the differential reaction of individuals to the same types of pressure. The latter is the first modification offered by most research directors; that is, they are willing to admit that while some individuals will do much better in one kind of pressure system, this may affect other individuals' creativeness adversely. Let us try to examine some of these assumptions in somewhat greater detail.

Pressure may be defined as a psychological (internal) state. "The individual feels compelled to do something" is one way of thinking about pressure of this sort. Now this compulsion may come from internal sources exclusively or it may be the result of external forces. Almost everyone agreed that in the last analysis the internal, self-motivated pressure was probably the most successful. However, some felt that the organization, through its policies and supervisors, could do a great deal towards building this internal state within an individual. Others of course felt that this would be wrong because it would not produce much more and might even result in a period of less creative work. In any case, one important question is, "How do you get people to feel compelled to do more and to do it more creatively?"

Pressure may be exerted internally because of personality needs, early childhood development, and so on. But it may also be exerted, and possibly sustained if internally present, by one's co-workers of the same status level, by fellow workers of lower levels, and finally by superiors within the organization. Pressure from each of these probably has somewhat different effects on different people, and very little is known about any of these effects. Perhaps more important is the question of how pressure is exerted by other people and it is to this that we turn next.

In those jobs in which a given number of units are turned out in a certain time interval, pressure can be stepped up by setting a norm at a rather fast pace. Where the end product is not standard, however, it is far more difficult to set a daily or weekly production norm. But it is possible to set up an equivalent mechanism—this usually takes the form of a progress report. If a progress report is required weekly, let us say, then a certain amount of pressure is felt to get something done in that time period. (Of course, many become expert at writing such progress reports to highlight a maximum amount of productiveness.) Other more or less formal mechanisms which have a similar effect involve counting the number of patents or patent applications for a given time period, the

number of publications, the number of papers delivered at professional meetings, etc.

The absence of pressure apparently can be found for relatively long periods of time in some laboratories. I have been informed by both the working scientists and the heads of these laboratories that there is no specific evaluation in terms of the number of papers or any other measure of productiveness. It is simply a case of "Doing the job well." The presence of pressure is somewhat more difficult to define insofar as the degree may vary considerably. It would be necessary to establish some sort of scale where perhaps the necessity for a weekly progress report combined with the use of many of the measures mentioned above would constitute high pressure, and an annual report in which only one of these measures is noted might constitute a low pressure. There is, of course, the additional complication that the actual number of any of these measures—that is, the number of papers or the number of patents—may vary from organization to organization as a norm; in one organization a single paper may be considered sufficient whereas in others a half dozen may be considered the norm.

Furthermore, it should be noted that one of the possible consequences of pressure defined in terms of number of publications, for example, may be to stifle creativity. This would occur when the individual scientist decides that the importance placed on the number of papers is sufficiently great to warrant turning out a number of "hack" pieces. Thus, the pressure of this nature may actually result in less creativity while on the surface it seems to bring about productivity.

The state of research and knowledge concerning the effects of pressure —as well as the very nature of different kinds of pressure—is such that it is not possible to state any definite conclusions. Beyond the pious hope that much more research will be done, very little can be added. What we have tried to do here is to indicate some of the kinds of questions that need to be asked about the effects of pressure on creativity in particular and then on productivity in general.

TOLERATION OF "ODDBALLS"

Research organizations appear to differ quite widely in the matter of tolerating "oddballs." Some of the research directors interviewed indicated that while they personally had nothing against oddballs, it would be very difficult to accommodate them in the context of their organization. The emphasis on, and the necessity for, teamwork in the organization is such that the oddball might not cooperate sufficiently and in fact might be a focus of dissension within the group. As one research director put it:

> The creative person is a slightly odd individual—he is an *odd*ball. I suspect that team work does *not* promote creativity and I suspect that too often people

are picked for their abilities to work together on a team in addition to their other technical abilities. We don't quite go along with this as we do have a few oddballs around and furthermore I don't agree that the oddball in a team necessarily ruins the other members' morale—which is presumably one reason why organizations with teams are supposed not to like oddballs.

The oddball is "different" but this difference is difficult to define precisely. He may like to work at night instead of during the day; he may balk at the most routine organizational requirements to fill out certain forms or to follow certain procedures in obtaining materials, etc. He may be considered odd in that he may not communicate anything about his work for months on end. He is sometimes considered a very moody person, introspective, not very "friendly." He may be considered odd because of the nature of the ideas that he suggests. Several examples were cited to me of persons who just poured out new ideas all of which were brilliant but slightly offbeat, with only one in ten or in a hundred appearing to be practical or worthwhile. The most frequently mentioned definition of the oddball, however, is that he does not seem to "fit" into the general organizational pattern. He is not, to use the new popular phrase, an "organization man."

The more highly organized the research operation and the more "teamwork" is emphasized, the less likely it is that the oddball will be tolerated. This is becoming an increasingly important problem as the notion of teamwork in scientific research grows in importance, especially in industrial laboratories. The problem is by no means limited to industrial laboratories, however, for it should be noted that the oddball is not tolerated with any more enthusiasm even in universities. Here there has been increasing emphasis on the ability of a prospective staff member to get along with the other members of the staff and to preserve the harmony of interpersonal relations in the department.

What do we mean by tolerating an oddball? As we have seen, there are some organizations where the directors would rather not have an oddball around for fear of destroying the harmony affecting the morale of the organization. When pressed, these research directors have replied in terms of the importance of the over-all effort of the research organization and the necessity for sacrificing the potential contribution of such an oddball for the "greater good" of the organization. There are, however, other organizations in which the oddball may be said to be tolerated, and these may be organizations which place a high premium on teamwork. There may be a deliberate effort to have the best of both worlds where most of the organization is highly organized and has a fairly high degree of conformity but where a relatively small number, anywhere from one to a handful, of oddballs are "tolerated." It is as if the organization were split in two and the oddballs just not expected to perform in the same way. If they cooperate with a particular team that is considered fine. If they do not cooperate that is perhaps not as good but the beneficial effect may

still be quite marked. They are not expected to produce as regularly as the rest of the staff and are evaluated in the light of quite different criteria. It is usually a "richer" laboratory that can afford to have or feels that it can afford to have a few oddballs. Their directors are willing to pay the price of keeping a potentially creative person in the expectation that it will pay off ultimately but they count on the vast majority who are not oddballs to make the regular contributions.

In still other organizations, the oddball is not only "tolerated" but he is encouraged. Of course, the degree of encouragement may vary. An oddball may be odd enough not to mind being considered odd in the first instance and not being particularly encouraged in the second. However, it would be interesting to know the extent to which overt encouragement may stimulate further creative effort from people verging on being "odd."

Toleration of oddballs is, of course, related particularly to the first factor mentioned previously, namely, the receptivity to new ideas, but it is also related to the second, the kind of pressures that exist in the organization. As one research director put it:

> I don't want a bunch of conforming scientists—I want differences of opinion and people who listen to one another. A "gutsy" atmosphere is stifling to creativity and confining to thinking.

By "gutsy" this director meant an atmosphere in which there was a lot of pressure to produce on a regular basis just as a factory produces. We turn now to the fourth factor singled out by research directors as important in encouraging creativity.

THE FREEDOM TO CHOOSE PROBLEMS AND CHANGE DIRECTIONS

Once said, this appears to be highly obvious, especially the freedom to change directions. However, this problem must be considered within the context of the organization and its goals. As already noted in the first section on the reception of new ideas, the nature of the over-all program and research goals of the organization may place limitations on the freedom to choose problems in the first instance and to change directions in the second. Where the end product desired is specific industrial applications, or where the research goals are restricted by the investment in and commitment to special areas of scientific research, the creative idea which is likely to alter these goals or prevent their attainment may not be greeted with the greatest enthusiasm.

The problem of being free to choose one's research problems is far too complex to be dealt with in this paper. The freedom to choose or change direction may or may not be restricted at any one of a number of points. The freedom of the research director himself to lay out specific research

areas may be restricted by the goals and policies of the larger organization of which he is a part whether this be industry, government, or the university. Within these limits he may set out general areas for research and allow senior scientists or laboratory heads freedom to follow up the project in any way they please. Projects may be broadly or narrowly defined. The scientist in charge may have complete freedom to follow any particular technique or course of action so long as he sticks to that project. Further down the scale we may find projects which are rigidly defined, where the techniques are closely specified. Something similar, or even more marked is the question of changing the direction once a particular project has started. Again, this may be related not only to the goals of the organization but also to more practical problems concerning budgets, availability of personnel, and general allocation of resources. In any case, the degrees of freedom with respect to a given problem may vary considerably from organization to organization, and within a given organization. It is suggested that creativity in an organization setting is related to the degree of freedom afforded the scientist to choose and change problems.

Without exception, research directors mention this freedom as a highly important factor in the over-all level of creativity in an organization. Each director tended to stress the importance of the particular level of freedom which existed in his own organization. But upon further probing, it became clear that many organizations were committed to a concept of *limited* creativity at best. That is, they would encourage creative ideas within fairly restricted limits of programs and projects and goals of the research organization. Finally, we turn to the fifth factor mentioned most frequently by research directors, namely, the problem of incentives.

INCENTIVES FOR CREATIVITY

No research director likes to admit that very little creativity is encouraged in his laboratory, and they all tend to stress the positive aspects of their search for ways of stimulating creativity. These directors like to stress the importance of the atmosphere that they have created and are maintining in the laboratory as one of the most important general stimulants to creativity. In addition, a number of other specific or limited stimulants are frequently employed. Among these special rewards might be mentioned increases in rank or salary, special bonuses, freedom to attend scientific conferences both in this country and abroad, as well as a host of other similar incentives. One of those frequently mentioned in industrial laboratories which should be noted specially is the concept of "free time." The general notion is that a scientist may spend up to a certain percentage of the organization's time and facilities, and sometimes even its money, working on any problem of his own choice. Hypothetically, this problem may be completely unrelated to any of the company's

or organization's choosing. Space does not permit us to go into the details of how this plan seems to be working. It is discussed in greater detail in another paper.[5]

Even a cursory analysis of the circumstances in which incentives are employed is very revealing. We find that in the case of free time, the scientist is nominally encouraged to use this free time for any problem, but the rewards go to those scientists who "come up with" usable, practical ideas. The working scientist may see free time as a diversion from the immediate problem, but if he comes up with a "good, practical idea," the payoff from this diversion is likely to be great; or, free time is used, not for an unlimited range of possibilities, but for possibilities closely related to the goals and research program of the organization. This immediately must place restrictions on the range of creative ideas encouraged.

Similarly, the range of creativity that is encouraged can be severely restricted when the rewards go only to people who have successfully pursued the practical idea, the idea suggested by the research director, or the idea that has paid off in a practical product. As noted earlier in our discussion of receptivity to new ideas, if the new idea is greeted enthusiastically but never acted upon, and if it is not rewarded tangibly in whatever manner of reward is customary in the particular organization, this may further inhibit creativity. In considering incentives and their relation to creativity, it is this relationship which is probably even more important than the nature of the specific incentives themselves. Judging from the literature, it is this very relationship which has been overlooked almost entirely. In other words it might even be better to have fewer incentives, tied more closely to creative ideas and creative research projects, than to have many incentives, none of which operates directly to encourage creativity.

Research is sorely needed to throw some light on the differential effects of the different types of incentives employed. In these interviews, especially with industrial research directors, there seems to be a strong feeling that money is a universal incentive (especially in our society). Without denying the importance of money as an incentive, it would be interesting to explore the relationship of the various possible incentives for scientists as opposed to general incentives used in other occupations. At certain stages in a scientist's career, incentives which can be translated into increasing prestige and recognition in the scientific profession may be at least of equal, if not greater, importance than increases in salary. To couple such incentives with increases in salary might be most effective. But it must be reiterated that we simply do not know which incentive or

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[5] N. Kaplan, "The Relation of Creativity to Sociological Variables in Research Organizations," in Calvin W. Taylor, *Proceedings of the Conference on the Identification of Creative Scientific Talent* (Salt Lake City, Utah: University of Utah Press, 1960).

which combination of incentives is likely to work best in different circumstances. There can be little question that in managing scientific personnel we have taken over some of the general assumptions operating in our society concerning incentives. The suitability of these assumptions should be examined more closely.

These, then, are the five factors which the research directors and many of the research scientists interviewed single out as important components of the organizational context which may encourage or inhibit creativity. As noted repeatedly, there is considerably less agreement on the degree to which each of these is important or even on the direction in which any particular one operates to encourage creativity. We may recall, for example, the discussion of the effects of pressure. Some research men were in favor of a high degree of pressure as an encouraging factor in creative research while others felt that a low-pressure atmosphere is considerably more effective.

We have tried to indicate some of the environmental conditions in the context of the organization which undoubtedly have some effect on creativeness in scientific research. Unfortunately, we have not been able to indicate how much effect or precisely what combinations of conditions are necessary to produce different effects. These, however, are factors which must be taken into account in studying the creative process in an operating research organization. There are undoubtedly many others which may be of equal importance. However, these five factors appear to be the more important ones which have emerged from our inquiry to date:

1. Receptivity to new ideas.
2. Pressure to produce.
3. Toleration of the oddball.
4. Freedom to choose problems and change the direction of research.
5. Incentives for creativity.

In closing, it may be well to emphasize again that to consider the creative process in the context of an operating research organization is a somewhat restricted approach. We look at the end product of many years of education, experience, and life in a society which has particular cultural values and particular attitudes toward creativity in general. To the extent that our educational system inhibits creative thought by discouraging the oddballs in the class, by requiring highly rigid study methods and checklist-type examinations, the educational system itself may inhibit further growth of whatever creative potential exists in the budding scientist.

To say that creativity cannot be studied in a vacuum is banal. But as a sociologist interested in the organization of scientific research and its differential effects—one of which is certainly differences in the level of creativeness—one remains handicapped in the attempt to use creativity as a dependent variable because of its lack of specificity as a concept. We need measures or indexes of levels of creativeness in our own type of study, and

it would also be extremely helpful to know the conditions in which different types of personality or different types of incentives affect different types of creativeness in the conduct of scientific research. In this, as in other types of prediction studies, we will find that superficial prediction through the use of predictors which we do not quite understand is inadequate. A deeper understanding of the problem is necessary. And to achieve this, both psychologists and sociologists will have to do a great deal of fresh and even creative thinking as well as much empirical testing of hypotheses and ideas.

Chapter 11

RESEARCH, DEVELOPMENT AND PRODUCTION: PROBLEMS OF CONFLICT AND COOPERATION*

TOM BURNS†

MOST PROBLEMS of organization present themselves in terms of personalities—as difficulties arising because other people are stupid, or hidebound, or self-seeking, or touchy. It is only when the same situations, and the same reactions to them and the same shrugging reluctance to penetrate personal antipathies and incompatibilities have been encountered in firm after firm that it becomes possible to detach the problems from their personal connotations.

This, at least, has been the writer's experience in studying the organization of some twenty firms and discussing his findings with their senior managements, severally in their own firms, and together at *ad hoc* conferences. The twenty firms—eight in England and twelve in Scotland—were made up of nine major firms in the electronics industry, six engineering concerns which had recently acquired some interest in electronics development and manufacturing and had invested some of their reserves in it, and five concerns in other industries with a stake in research and development. The studies made in each firm were centered in two topics: the general management problems peculiar to firms involved in rapid technical progress; and the particular problems of obtaining effective working relationships between research and development groups on the one hand, and production and sales groups on the other. The present paper presents some of the material and findings which bear on the particular topic of the working relationships between R & D and manufacturing.

The organization of an electronics firm has to overcome obstacles presented by the different levels of scientific and engineering attainments prevalent in different sections of the firm. These differences are overlaid by considerations of prestige and of control over resources. However, the translation of jobs to be done from the language and methods appropriate to R & D engineers into terms manageable by draughtsmen, production

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engineers, production supervisors, and, eventually, operatives, remains a central task of the organization. Similarly, of course, the difficulties and problems encountered in manufacturing terms on the shop floor have often to be translated into the language of the laboratory—and, indeed, have to be recognized as so translatable by the manufacturing side.

Such problems I have labeled linguistic. When management thinking follows orthodox bureaucratic lines, it tries to take care of such problems by multiplying the ramifications of an organization chart.

INTERMEDIARIES AND INTERPRETERS

The usual response to difficulties of passing designs through from R & D to production—correctly read in most cases as a problem of interpretation—has most usually been the creation of special intermediaries, whose job it is to interpret. There exist, in many of the concerns, groups of highly trained people whose existence depends on the continued existence of these difficulties.

In one concern, the number of specialists acting as connecting links had grown rather formidably. The liaison structure began with sales engineers from product divisions resident in development sections. The model shops had been placed under the administrative control of production, the growing need for precision mechanical work in them being met by the introduction of methods engineers (from production).

After development had produced a satisfactory prototype, a job passed to the stage of production drawings. At this point again, production was involved, this time operationally, through more methods engineers resident in the drawing office. They "supplied" production engineering technique to the designers and draughtsmen who produced drawings to the instructions of the development engineers. A fourth element which acted on the drawing office was the Standardization Group, which endeavoured to limit variations in the specification of components and the design of parts. This group was also "resident" in the Drawing Office. The draughtsmen's job, therefore, was to see that drawings were produced by the date required by the product division, in accordance with the design group's specification, within the limits set by the standardization group, and in conformity with the ideas of the methods engineers. Quite apart from the organizational complexity which this involved, detaching so many constituent functions from the draughtsman's task weakened his sense of involvement and his interest in his job. His responsibility for seeing that drawings and schedules really did provide all the information they should, and were up to specification, could be surrendered to the supernumerary controllers surrounding him. He became reduced, in fact, to what the managing director said he was: "just a chap with a pencil."

Fragmentation of the process of designing and preparing for production continued in the next stage. Drawings, each signed by a D.O. methods

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engineer, passed next into "Production Engineering" (planning), and production prototypes were then made in the preproduction shops. At this point, discrepancies often appeared between the performance of equipment manufactured to drawings and laboratory prototypes, discrepancies which could arise merely from the cumulative electrical distortion of parts and components which are individually within specification, or from misreading of design requirements at any of the preceding stages, or because of necessary adjustments made by engineers or model shop craftsmen to formal specifications and designs which are not entered on drawings. The traffic generated by these contingencies was again carried by methods engineers located in preproduction, who acted as intermediaries and interpreters, as well as problem-solvers. A similar organizational arrangement was being built up in the production shops proper, with project engineers "taking care of troubles arising with test, drawing office and development."

Development, finally, had produced its own liaison specialist with whom to confront the liaison specialist from the plant. "If there's a design going through production for which we've been responsible, well, a project engineer represents what happens in the plant as far as we're concerned. He'll come to us with a tale of woe, and get on to one engineer in our group—the post-designs engineer. He may be able to solve the thing on his own. If not, he goes to the particular engineer who was responsible for that bit of the design." The plant, he hardly needed to add, was a *terra incognita* to him.

This, the most extreme example, is, however, only one instance of a widespread response to the problems of sheer translation of information which arise between people in different phases of the total system. This might be described as a problem in linguistics. It is also an attempt to adapt the present management system to the problems posed by technological innovation rather than change the management system to a more appropriate form. The creation of special intermediaries and liaison groups seems to offer the possibility of retaining the previous definition of function and of lines of command and responsibility.

Yet it was not the manifest aim of management to revert to a stiffly bureaucratic system. The head of the concern in which this state of affairs existed introduced his outline of the organization by saying that he, like the chairman of the group of which his concern formed part, was strongly in favour of giving jobs as little specification as possible. Doubtless, when every new group was created, a perfectly sound reason had appeared to justify it and to show why the shorter circuit would not work. While it is not possible to point to the reasons in specific instances, I am inclined to ascribe the tendency to two characteristics of managerial thinking. The first is the attitude to look for the solution to a problem, especially a problem of communication, by "bringing somebody in" to deal with it. A new job, or possibly a whole new department, may then be created

which depends for its survival on the perpetuation of the difficulty. The second is the attitude which probably derives from the traditions of factory management, which cannot bring itself to believe that a development engineer is doing the job he is paid for unless he is at a bench doing something with his hands. A draughtsman isn't doing his job unless he is at his drawingboard, drawing, and so on. Higher management in many firms are also worried when they find people moving about the plant, when individuals they want are not "in their place." They cannot, again, bring themselves to trust subordinates to be occupied with matters directly related to their jobs when they are not demonstrably and physically "on the job." Their response, therefore, when there is an admitted need for communication, is to tether functionaries to their posts and to appoint persons who will specialize in "liaison."

One of the consequences which follows upon the confinement of movement and interaction by the creation of specialist interpreters is the growth of isolationist sentiment. There was, in our last example, a noticeable tendency to speak in terms of functions as quite detached from the commercial purposes of the firm; jobs were spoken of as discharged "under pressure" from other groups, or "as a service" to them.

SOLVING THE LANGUAGE DIFFICULTY

In general, the fewer the links in the chain from development to production, the more development and production are forced to learn each other's language, and the more effective, speedy, and trouble-free is the passage of designs through the various stages.

This was revealed with particular force in the one sector of the industry which, significantly, is wholly devoted to mass production: the radio industry. When problems of the relationship of design to production appear at all, they take their place among a variety of minor endemic worries. Domestic radio manufacture tends to have few stages and plenty of interaction between them.

Outside radio design and manufacture, relatively effective relationships appeared to exist in two concerns. Both were very young companies, and may have been still dependent on the sense of common purpose and the loyalty and enthusiasm which almost any pioneering activity can command among a group of people, especially when it is successful. Nevertheless, the problem of mutual understanding and adequate translation was being treated in both cases in terms that were more direct and more sophisticated than elsewhere. This, it seemed, was partly because the time when "things worked beautifully and everybody pitched in" was so recent that when new problems appeared, people set about analyzing the problems, and asking why the original system no longer worked. The factory manager of one of the concerns gave this account of the origins of the present system:

> I used to live next door to the development director. The kind of thing that happened was that, say at nine o'clock one evening, he'd get something down on a piece of paper. At ten o'clock I'd be with the pattern maker. At eleven I'd be talking to the production engineer. Next morning I'd be talking the thing over with my production people. By twelve we'd be looking at production resources and seeing where it would go in the programme. Well, as we grew, a lot of people were in the place trying to use the same technique without having the ability to put it through. So you'd get somebody outside—usually the accountants—insisting on coming in and formalizing the system by getting it documented.

Eventually, some two years ago, the company engaged consultants to devise procedures for controlling the passage of designs through production. This is now referred to as the "paperization phase" and is almost universally remembered as disastrous.

> They introduced a terrific amount of delay into the system. I suppose consultants are incapable of thinking about production in terms of tens and twenties off. . . . It resulted in design groups, which were not affected by all this, continuing to produce drawings as before. These would get involved in a flow of paper from Banda machines. They (the design groups) would be called on to produce more and more drawings of sub-assemblies, etc., so that you'd get the process of piece-part manufacture and sub-assembly starting and stopping four or five times over, the stuff going into stores in the interval. This was very frustrating. . . . But we're beginning to see our way through this. . . .

Although during the "paperization phase" there was an attempt to interpose a clear handover frontier between design and production, with no works order issuable before a design was completely drawn, the system later regained its elasticity. Production engineers were attached in the early design stages to design teams. On occasion, they might be responsible for the production of the salable prototype in the preproduction shops; they were further responsible for the production of a first pilot batch. This section had been given a rather higher status than is usual, and was regarded as the repository of manufacturing "know-how." They worked directly under the factory manager, who spoke of them as "opposite numbers" to design teams. Their relationship with preproduction shops appeared to follow that normally existing between laboratory and model shop. Production engineers, preproduction workshop and planners "lived close together" so that preproduction skills and experience with a design could infect the planning activity.

PRODUCTIONIZING DEVELOPMENT

One of the curious features of the whole study was the discovery of the more primitive diseases of bureaucratic structures flourishing inside laboratories. Development laboratories were much more preoccupied with formal organizational structure than were research laboratories or even the production side. In all eight English firms, some twenty-four physi-

cally distinct establishments (*i.e.*, research, development or production) were visited. In only four of these did anyone refer to an organization chart; all four were development laboratories. In one laboratory, the organization chart revealed five distinct ranks, apart from technicians, who had three grades. (The existence of so many levels was, it is true, said to be a "problem.")

The experience of working inside one large development establishment organized into project groups, subgroups, technique groups and service sections, was described in these terms by one development engineer:

> You need to use such a lot of personal energy in order to get anything at all done. Take a simple example. . . . You want to make a piece of equipment you're developing. All right, you make some sketches, or get some sketches done, and then you take them along. You can't take them along to the model shop foreman and say, "Here you are George, get these done for me." It has to go through the central planning organization. You take them to the central planner and immediately his reaction is, "Look, I can't do those for three months. If you want them done in six weeks you can get them done in six weeks but you will have to go to someone and get priority on it." The general upshot is that you spend one day, or perhaps two days, arguing with first one person then another, and as a concession you would probably get somebody to agree that you could have them done in a fortnight. It wouldn't make any difference, it would still take six weeks, but you would have spent two days arguing about it. Well, you get your bits and pieces made up and then you go to test them. Now I want some test gear. Can't find any test gear. Go and see the lab steward. "Ah yes, what do you want, 'scope, signal generator? I expect we can find that for you," and you spend a morning looking round various labs, and you find the 'scope. You go to dinner, you come back, and find somebody has taken the 'scope, but in the afternoon you find the signal generator. . . . It really makes me wonder how the firm operates economically. There is an awful lot of time wasted in doing things that are unnecessary. The test gear situation is largely generated by unwillingness to spend capital, I think. . . .
>
> In a large organization, too, you find that a project gets divided up and subdivided up to a very large extent. You will find one lab interested in making the pre-amplifier. Another lab is interested in making the output amplifier. Somebody else fits those two together into an amplifying unit. Then yet another group in another room will fit that amplifying unit into the final equipment, whatever that is. Of course, that sort of thing happens on the production floor, but on the design (development) level it does tend to make communication difficult.

This is a one-sided view. Even in such situations, more than one laboratory chief has since pointed out, there is almost always some other work to engage the attention of the individual engineer while he is waiting for essential parts or test equipment. Yet the account does reveal the tendency, which has gone quite far in some concerns, to "productionize" development-design organization, *i.e.*, to break down the total process into stages akin to those of manufacture.

Sheer size and the traditions of industrial management make for attempts to dissect the laboratory into sections according to one of two broad principles: "teams" may be created to develop a whole system, or

"project"; or specialist technique groups may be formed each handling a part of a project. In either case, there are residuary "specialists" or "project" teams. Neither altogether successfully avoids the wastage of effort by one group's duplicating the work of another.

Some of the peculiar and vital organizational problems of the industry concern the disposition of development teams. The problems of balance, of emphasis, and of making groups elastic in size are always and inevitably present. Great efforts may be expended in building up the design teams of a laboratory division to adequate strength, with the virtual certainty that after a few months there would not be enough work to keep all the engineers busy. It is not uncommon, within the same company, for one laboratory group to be grossly overloaded and despairing of completing jobs in time, while another is obviously short of work and searching for slightly esoteric technical problems on which to keep going; and this, even where it could be met simply by the direct transfer of engineers from one group to the other.

Research laboratories, one might observe, were distinctive in their internal organization, teams being associated with techniques rather than with projects or products. It was said that the main difference between development and research was "in the way we think. We (Research) don't think in terms of a product, but of a department of science." In the same research laboratory, internal organization was said to be "loose." This meant, apparently, that while there were differences in seniority, each individual tended to regard himself, and to want to be treated, as a researcher. This made any organizational hierarchy impracticable; there had to be direct and easy contact between the head of the laboratory and each individual.

In many ways, a special ethos of scientific professionalism was cultivated in research laboratories. One "essential" difference cited between research and development people was that the former always wanted to take a problem to a "final answer," whereas the development engineer might be quite prepared to surrender his part and interest in a project and switch to another. Again, it was "policy" in one research laboratory to encourage publication of papers as a form of production proper to a research establishment, and perhaps as prestige advertising of a kind familiar in universities. In another, there was a "policy" of trying to see that each technique group had its own technical study project, this making for continuity and allowing "scope for original creative work."

The linguistic and structural problems of organization which have so far been recounted were everywhere marked, complicated and exacerbated by dissents over claims by laboratory enquiries to a quasi-elite status, and over their attempts to gain greater political power within the concern.

The industrial scientist is now a member of a distinctive group in the national population, rapidly growing in numbers (which are nevertheless

perpetually short of national needs), accorded a very considerable share of public and political attention, and inevitably conscious of the importance and the power attached to the technological contribution. This awareness has its effect on the general demeanour of the industrial scientist, particularly in his dealings with industrial managements, through whom the clamant needs of society for his services are expressed and who are the media by which these highly valued services may be translated into effective action.

To this general influence on the position and demeanour of the industrial scientist, two other relevant factors can be added. More perhaps in Great Britain, where the initiative in the expansion of technology has lain outside industry, than elsewhere, the industrial scientist conceives himself as a member of a professional group, the bounds of which extend over the institutions of higher education and government service as well as of business.

ISOLATION

The simplest and most direct consequence of the special status ascribed to the industrial scientist is that in extreme cases he becomes so detached from the rest of the concern that it derives little benefit from his presence. This was the situation in one of the Scottish firms. It had been devoting a good deal of energy and money to the exploration and development of new markets. A year or two before we became acquainted with it, an industrial scientist was brought in to take charge of a "Research Laboratory." In addition, the firm had the services of academic consultants, and was supporting research in a University department of Engineering.

"The main idea in starting the Research Department" said the industrial scientist, "was that it should pick up on development work which other firms in the industry seemed to be pressing on with." This development work included electronic control devices in which notable advances had been made by one or two major competitors, but the projected scope was much wider.

At the end of two years, virtually no development work had been initiated in the laboratory. The small "R & D" team had become instead a plant testing station and consultant on production problems. The main feature of the laboratory was the test gear, which the firm had decided to install after the appointment of the research manager, who had been responsible for a similar, though much larger, plant in his previous job. "There was no intention of getting this plant in when I applied for the job." He was involved in testing throughout the production process, and spent a good deal of time on incidental problems of the design and manufacture of items of current production referred to his department. When we suggested to senior management, after some time in the plant, that a tendency seemed to have grown up for the factory's design and

production departments to unload on to the research group problems they would formerly have tackled themselves, they agreed that this was so.

There seem to have been two main reasons why the development laboratory found its work diverted from what it had been created to do, and its growth stultified.

1. The first was the existence of a backlog of development problems arising from users of their products. The industrial markets now being served by the firm were continually throwing up needs for improved types of equipment. "Over the years the firm's collected a number of jobs we've quoted for," said one senior manager. "The policy has been that whenever the managing director has had a letter from an outside customer enquiring about something not in our range, and he thought we might do something with it, he'd insist on quoting for it. The same with tenders. Well, we'd do this 'with a pen and a piece of paper' and send something in. Usually we never got the order. If we did then everybody would be running around in a panic getting drawings done, trying things out and so on. The upshot was that we acquired a long list of things which at one time or other we'd decided we might try." Before the advent of the Research Department, user problems of this kind and others arising more directly out of the firm's experience with customer's needs had been a general management task; in particular, the managing director's considerable flair for production design had kept the list of outstanding items turning over, albeit more slowly than the rate of arrival of new problems. After the research department was set up, the managing director maintained his practice of attacking this list of items at places where he could see some possibility of a profitable solution, but without much regard to what the research department was doing.

"When they (the 'Research Laboratory') moved in, the managing director was very eager to come in and do a lot of work. 'Now,' he said, 'I can really get down to the things I'm interested in,' and they reserved a small office for him upstairs and he came in and supervised the finishing of it, got all the pencils sharpened on his desk and so on. He's never been back there since." The number of items on the list, their specific nature, the firm's tradition of "trying its hand" at problems posed by actual needs arising in the market, and the tendency to use the Research Department as a technical reinforcement, all combined to direct the main exploratory effort of the laboratory in the same direction. "There's no policy about development work as against this dealing with items and small enquiries." So the position was that there were two fairly distinct attacks being made on the list of development ideas derived from market contacts. They were conducted at different technical levels, and the angle of approach was very different; the oversight exercised by a tightly organized senior management group was quite sufficient to avert waste of effort through repetition. But the creation of the Research Department did not, in fact, introduce any innovation in the firm's products or manufacturing methods

of the kind explicitly looked for when the Research Department was planned. Instead, the added resources were used to reinforce the previous efforts to improve design and meet customers' special needs, and to supply better test facilities.

At one level of explanation, the sheer momentum of the firm's previous practice in building up and working on a development program may be said to account for the distortion of the working organization needed to operate the new plan for development work. Yet the firm had a bent for expansion, for taking on new functions, attacking new markets. The working organization proved itself, time and time again, extremely flexible and readily adaptive to new or recurrent needs arising from other sectors than research and development work; indeed, it is this firm which has provided us with some clear and important illustrations of the working of adaptive organic procedures in organization. The questions remain, therefore, of why the distortion took place, but now take the form of why it was that the organization of research and development *in particular* remained stable despite a conscious effort to change it.

2. A number of circumstances seem to add up to sufficient evidence for assuming that the second and fundamental reason lay in the peculiarities of the status system.

A great deal of emphasis was placed by senior management on the possession by all members of the sales, engineering and production staff, of a common language, common background and common level of technical competence; almost all had qualified in the same technical college. The management staff, therefore, although at different status levels in the concern, nevertheless possessed a common professional status. This was somewhat lower than that of the Research Manager.

The Research Manager, after two and a half years, was the most isolated member of the top management group (of thirteen members). This appears from a study[1] made of the actual expenditure of time by each of this group according to seventeen subjects and according to the persons with whom they were in contact. The Research Manager was engaged in the smallest range of subjects, apart from one senior office manager. He communicated with the smallest number of other departments. He spent the least amount of time with other managers near his level or above, apart from the chief foreman. When each participant in this study was asked to ascribe a rank relative to his own to each of the other participants, most difficulty and most difference of opinion was found in fixing the position of the Research Manager, who was finally given, by agreement, a position of his own below that of the directors but above the seven heads of departments who were of similar age.

The research department alone was physically separated from the rest of the plant by a wall.

[1] T. Burns, "Management in Action," *Operational Res. Quart.*, 8: 45–60 (June, 1957).

The reason why the momentum of previous practice in the firm seemed to apply to the research and development programme and not elsewhere appears to me to lie in the circumstances just outlined. They point to a rupture between the Research Manager and the rest of a tightly integrated management. This rupture is closely identified with the distinctive status which attached to his possession of rather higher technical qualifications and standing than those common to the rest of the management.

In the concern which figured in the previous section, there was an actual brick wall between the laboratory and the rest of the concern, the only barrier in the plant. In other places, the "brick wall" which was mentioned in interviews and consultations was metaphorical, but even so was at least as much a barrier.

In a large research and development organization, employing scores or hundreds of industrial scientists, there is ample room and opportunity for the cultivation of the development engineer's self-conception and of the claims to special status which arise from it. Conscious of the social and economic significance now ascribed to his qualifications, linked by training and occupational interest with colleagues outside industry, credited too, perhaps with some of the supernormal abilities and abnormal mannerisms which have figured in popular myths about the scientific inventor, the industrial scientist is under some pressure to regard himself as apart from the run of salaried employees in industry.

If he subscribes to this view of his "apartness," then the industrial scientist will incorporate in his conduct a number of signs which will separately and in combination demonstrate this to other participants in the occasions of his working life. Starting work an hour or so later than the production and office staff is one such sign; it derives not from assertion of an occupational inability to get up early as from the general specification of scientific work as erratic and subject to varying rhythms of activity and to bouts of enthusiasm which, by compensation *may* keep a worker in his laboratory into the small hours or over the weekend. The cultivation of eccentricities in dress and in leisure pursuits serves the same end. Superficially, that end appears to other people involved as the maintenance of a social status superior to that of their "opposite numbers" in the rest of the cocnern. Thus, the head of one concern, interpreting the conduct of his research and development engineers in terms of ordinary social differences based on educational achievement, wrote, "The Laboratory staff, having graduated from a University, are naturally individuals of a higher intellectual standard than the shopfloor personnel and they have a tendency to consider themselves as superior beings and to act accordingly," and "designers tend to be young and rather arrogant."

The cultivation of marks of conduct which could earn recognitions as a member of a kind of industrial elite does, of course, have its own reward. However, some special significance attaches to the recognition of such conduct as peculiar to industrial scientists, rather than to everybody who

enters industry after graduating at a university. We are not, that is, concerned merely with a series of isolated exhibitions of aloofness or snobbery.

This deliberate avoidance is founded on a sense of social incompatibility which contrasts strongly with the consensus about interests and evaluations of people and aims which prevails in the laboratory group. Interviews with members of one large laboratory contained a number of reflections of the contrast. The ease and friendliness which existed between development engineers at all levels of seniority was immediately apparent; Christian names were used between the most senior and junior members, including technicians. They had their own recreational groups and their own informal social activities outside the factory. As for relationships with the factory, "they are pretty formal; that is, they're confined to functional relationships with other departments. We have nothing to do with the machine shop, for instance. Of course, there have been one or two efforts to get us all together on do's arranged by the firm, but how can you be said to have any sort of social relationships with chaps who are only interested in the dogs and in football matches? It's pretty hard to keep up a conversation at all." . . . "Oh yes, a few of the chaps in the lab live near us, and we see quite a bit of each other in the evenings." This was from a senior member of a design section. A junior member said, with emphasis, "There is no contact with the factory—no personal contact, that is. In the lab, we're very happy—sort of happy family relationships. The lab chief must select people on the grounds of getting on with others; they certainly do get on with everybody . . . we have regular meetings with Ministry people and other Government establishments and quite a lot of which you might call social contact with them too." Another: "Personally, I keep clear of the factory."

This last informant went on to give accounts of two occasions on which he had outmanoeuvred a production department and an office department by getting possession of their data surreptitiously and producing results they either could not or would not produce themselves. Naturally, these activities had been conducted on behalf of his laboratory section, and with the approving knowledge of his seniors; no more had been involved than securing the time sheets of a production department to prove a point in dispute about the allocations of hours to a particular project, and the preparation of an estimate for a development and production contract without consulting or informing the factory costs and estimating department, but the effort scored points, for the speaker and those present at the time, not so much in the interests of the concern's effectiveness as a whole as in the perpetual design vs production campaign.

One inevitable consequence, in this concern, of the display of attitudes and of conduct such as we have related was a blockage of traffic in the other direction: from production to design. There was at least one case of

a technical problem being referred to a Research Association in London, when the solution might have been found by consulting a few specialists in the laboratories a few yards away from the production department, and under the same roof. So marked was the reluctance among production engineers to consult laboratory engineers about problems incidental to production—to use "scientific brains"—that top management entertained dark suspicions about "instructions" being given to stop them. Instructions, had anybody been foolish enough to give them, were entirely unnecessary.

In another concern, production managers reacted more directly and vocally. Objections were raised to the laxity which prevailed among its development engineers over rules which members of production departments had to keep whatever the status—especially punctuality. Production managers raised a strong objection, as did development engineers to the proposal that they share a new building. Their present establishments were miles apart, the development engineers in an elegant, pleasantly situated building, surrounded by lawns and gardens and attractively decorated and furnished. The production shops and manager's offices were, by contrast, crowded and makeshift. "It's often been suggested," said one production manager, "that we should make up a party of our people and take them over to see around the labs—good idea to let them see what the firm is doing in that line, and so on. Well, that party hasn't come off and won't. We daren't. What the eye doesn't see, the heart doesn't grieve over."

The social barrier between development engineers and others made itself felt in every single firm. Even in the concern in which they seemed least obtrusive, a senior engineer mentioned an occasion when his team had been spending the morning working very closely with draughtsmen and he had—either absent-mindedly or by way of a gesture of solidarity—joined them at their table for lunch. As he left the canteen, which development engineers shared with draughtsmen and production supervisors, he was asked, "Been slumming today?"

No concern, it is safe to say, is without political or social conflicts which generate, or contribute to, manifest inefficiencies of communication within the working organization. What we have been considering is a situation of a special kind, the distinguishing features of which are all aspects of the special status claimed by technologists.

The consequences of this situation for the working organization appear in the maintenance and reinforcement of the "linguistic barriers."

In an industrial milieu such as electronics design and manufacture, new problems, unfamiliar procedures, materials, parts and assemblies are very frequent. This means that individuals are not only constantly having to explore new ground, they are being asked—and having to ask others—to do things which are not written in to their job specifications. They must therefore be continually having to ask others for information or assistance

beyond what has been provided before. In the circumstances obtaining in all the concerns we studied, for production people to ask designers what is meant by an unfamiliar feature of a drawing or specification, to ask whether an alternative method of making is possible, and so forth, might well carry too big a risk of a snub or of adding to the body of criticisms of production which had been built up.

People in industry have, like the rest of us in other circumstances, tended to reconcile themselves to living with the situation rather than trying to alter it, blaming the "System," the cussedness of things, or the equally inalterable disposition of the kind of people which surrounded them. There was a curious readiness to speak of "physicists" as "a different kind of animal," for the head of a laboratory to say to a managing director that he "really can't expect lab people to observe the same rules of punctuality as other people because they just aren't like ordinary people," for a production manager to say that "My people are the sort that stick to routines in life; their whole outlook on life and the way they think people should act is different—miles away from the R and D lot. They're temperamental, we all know; they look differently at things. Put them together and you'll always get clashes. The only thing is to keep them as far away from each other as possible."

"Clashes" occur because the special invidious distinctions between the status of individual scientists and other members of a concern are not accepted as "legitimate," *i.e.*, as in conformity with the expectations and obligations normally attached to membership of the working community.

This is not the whole story. Many entrants to working communities bring with them special social or educational advantages. The second peculiarity of the distinctions we are examining is that they are relatively new in the engineering industry (the indications are that chemical industry has long accommodated itself to them); also they are presented as an organized affront to accepted conventions. They also indeed threaten the accepted social structure by being attached to a new institutional form—the Research and Development laboratories—which is a contestant for political power in the firm.

The following incident demonstrates how the subordination and eventual disruption of the whole relationship between R & D and the rest of the firm can be brought about by the single issue of internal politics.

An industrial scientist with high qualifications and some reputation in the industry was brought in to take charge of a laboratory group. The firm was old-fashioned, and had decided that it must build up technical resources which would render the firm independent of obsolescent products and methods. After two years' experience with the firm, the head of the laboratory said:

> The understanding was, that I was needed by the firm, I was to take over their whole technical side and be responsible for technical development. My view now is that this has been a wholly unsuccessful venture as far as I am

concerned—or the firm. . . . Everything I've put up has been blocked, it's met with deaf ears. As far as I can see, the firm just hasn't been able to see what to make of me.

I've had several propositions which I've put up to the firm, none of them has been accepted. It's been their view that whether or not such and such a development takes place is just not my affair; this is the manufacturer's concern, and, similarly the question of how something is to be developed or produced is regarded as not my affair either. So far as I can see, what they expect the lab to do is to hand over a blueprint of a new product, which the manufacturing side, the plant manager, could put into production almost straight away. . . . They have no conception of what development work in this industry involves . . . they constantly refused to put aside a certain amount of capital each year for development purposes.

The basic political issue was again the expansion of control by the laboratory group into sectors of the concern previously under the exclusive control of production and higher management. It had, however, been exacerbated by actions of the research manager; soon after his arrival, he had submitted a report on the equipment and operating methods of the productions departments which had been extremely critical. From this beginning, relationships between the laboratory and the rest of the concern appear to have flowed in ever narrowing channels. From the laboratory came reports; criticisms of faulty techniques; demands for new equipment; proposals for intervening in the running of the manufacturing process to prove laboratory trials, all of which were construed as arrogant and irresponsible. From the other side came refusals to embark on any change or new product except one which would be a guaranteed success or would cost nothing; this was construed as obscurantism and meanness.

The research manager had recruited a small team of industrial scientists, including some three or four graduates, but the effective work of the group was confined to technical control of the product at various stages, including the testing of raw materials and products. This had, of course, been done before his appointment and the buildup of the laboratory group. It was now done rather better.

Three years after his appointment, the research manager resigned. The situation had reached a point, before the end, at which the research manager could say that he had "not spoken one word for over a year" to the managing director, whose office was next to his. It should perhaps be added that the research manager had been invited into the firm by the same managing director, with whom he had been on friendly terms for many years.

Here, as elsewhere, the only factors contributing to the situation which were visible at all to the people directly involved were personal. And, as the situation confronting each person grew more intractable, so his own aims and their frustration became more charged with emotion; so that in the end, associations and conflicts between the people concerned had in fact been translated into personal terms.

POLITICS AND THE SHAPING OF THE WORKING ORGANIZATION

Perhaps the most interesting and enlightening political campaign encountered in these studies was that successfully conducted by a Development Laboratory to establish its right to separate autonomous existence between a Research Laboratory and the Production plant. All of these were situated in different towns, although the intention had been to put the Development Laboratory and factory on the same site. Work had begun in the Research Laboratory which had been part of the parent organization; the Development Laboratory and, some four or five years later, the factory, had been newly created.

While the distance between the two laboratories took some time to cross, it was also seen as an advantage by development engineers. There was little doubt in most people's minds that separation made much easier the independent critical appraisal of the products of Research and the ability to decide on redeveloping. It was said, for example, that Research began by believing that the Development Laboratory would merely take over a design that they had produced, check the tolerances, see that it could be engineered, make a prototype, and send it for manufacture. But this "doesn't often happen; more than likely it has to be entirely redeveloped. So Research takes on study contracts, and we produce something that will sell and meet specifications—something that can be made as economically as possible, will continue to work, and so on." Even when remoteness was not seen as a positive advantage, the drawbacks were regarded as negligible—"We speak the same language and there's a good deal of frank talking on visits. . . . We know what they're doing very early on." Information has to reach a certain level of importance before it becomes worthwhile going to the trouble of communicating even by telephone; "this means that there exists something about which a practical discussion can take place." Separation also tends to face Research with requirements for specific jobs of work—"instead of an idea just growing and growing." With Research and Development in the same establishment, it is often impossible to get a design cleared up stage by stage as the tendency is to say "hang on, we've just got a new idea," and this tendency can work right through the process. Lastly, separation and the fresh scrutiny which a project can receive when it passes from research to development may "prevent rogue ideas going right through without being stopped."

Many of these statements reflect the growth of a quasi-inspection function. "Relationships with the other lab weren't at all good three years ago. They couldn't understand why their brilliant circuits were criticized. Now they know by experience that they can go wrong."

A number of informants in the Development Laboratory recalled difficulties in cooperation with the Research Laboratory during the first

years, particularly because of the unwillingness of Research to accept the fact that development groups had a significant part to play. The improvement which came about was ascribed by Development engineers largely to the difficulties which occurred in the manufacture of prototypes. Research became willing to let Development sort some of these troubles out. Status distinctions between the two groups also became less obtrusive. "Most of the troublesome people have left—you really can't have research people who think they're God Almighty."

Over a number of years, the Development Laboratory struggled to insert itself between Research and Production until, in 1957, a balance was struck. Titular recognition was given the Development Laboratory by coming up with an identical name for both the Research Laboratory and the Development Laboratory, and by appointing the heads of both as Joint Managers. This also set the official seal on a system of pairing between the "opposite numbers" working in either establishment on the same equipment, or technical item. This system was extended down through both establishments and was acknowledged operationally not only in informal contacts but in the membership of all progress meetings. The chairmanship of the meetings was alternated between the groups.

But an almost parallel situation prevailed between Development and Production, which had been created later, and had been located a good distance away. This factory had begun with dreams of being "a genuine production factory" and still strove for autonomy. Production pressed its claims for autonomy by demanding clear designs and specifications and by claiming the job of translating these into manufactured articles without Development retaining responsibility.

Many of the moves and ideas which figured in the account by development engineers of their group's struggle for autonomy and equality were repeated, with the significance reversed, in the account of relationships between Development and Production. However salutary the influence exerted by a separate and remote Development laboratory on Research might be, having Production so far away was "thoroughly bad." It was much better, said the development engineers, for the designer to "live with the job"—to have design responsibility for equipment throughout production. Only by having development on the spot with production was it feasible to maintain this kind of control; otherwise there was a tendency for a handover of responsibility to take place. "What happens is that you're constantly getting unsuspected faults arising from characteristics which you didn't think important in the design. If you hear of these through a . . . production person to whom the design was handed over in the dim past, then instead of it being a design problem, it's an annoyance caused by that particular person. You thought you were finished with that job and it's an annoyance because now you're on to something new."

The whole two-sided situation is neatly expressive of the way in which

political pressure is rationalized in terms of organization. The reversals of argument were taken even to the point of explicitly suppressing pair relationships (so valuable in the build-up *vis-à-vis* Research!) which had grown up between development engineers and production people. "We've set up a production liaison chap to act as a channel for communication. You can get all kinds of private arrangements between production and development people . . . personal knowledge is no good to anyone." The overt expression of the Development laboratory's campaign for power and status equal to Research and for control over Production was made in terms of the exigencies of organization and had been by moves made in the interests of more effective working relationships. The structure of relationships between the three establishments was, in turn, the creation of the successful manipulation by the Development Laboratory of its intermediate position to establish first its necessity and finally its leading role in the whole tripartite system.

No firm was without some serious political conflict dividing it. It need not, of course, exist between departments, and perhaps the most interesting of all the firms were those in which political divisions cut across the functional divisions. In many of the Scottish cases we have quoted, production had obviously stood for the previous order of things, and the laboratory groups represented the new dispensation. In one English company, the same conflict of old and new visibly underlay the political split *within* a large design department. The issue there lay between those working in technical fields which had remained basically unchanged for a generation and those recruited for new electronic work, which, it was expected, would eventually replace the older techniques. But, typically, the main division observable in the larger electronics concerns was between sales and development. Also, on a closer view, it seemed that the sales-development conflicts had become noticeably acute in the last year or two, with the increase of sales effort in the new commercial market.

What is striking about all of these kinds of political conflicts is that they were allied to the appearance of a new group, or the rapidly enhanced importance of an existing group. This threatened the power, influence, and prestige of the formerly dominant groups. The political issue between sales and development was, moreover, particularly acute where the growth of sales had been accompanied by re-organization into product divisions, in which the leading role fell to sales.

Thus, the main political issues in most of the major electronics firms seemed to arise from the resistance of development and design engineers—the professional innovators—to an innovating change, much as production people in the smaller firms had resisted development engineers as the instruments of change. But political issues arise not from the fact of change itself, but from the identification of change with one section of the concern, whose new role and expansion threatens the power and standing of other sections.

Chapter 12

THE IMPACT OF ALTERED OBJECTIVES: FACTIONALISM AND ORGANIZATIONAL CHANGE IN A RESEARCH LABORATORY*

PAULA BROWN AND CLOVIS SHEPHERD†

THIS study will describe what happened when a naval bureau in Washington changed the objectives of one of its laboratories on the West Coast. The change was, in essence, from applied research to development. If the personnel of the department had behaved as "obedient employees," they would simply have changed their behavior to conform to the new policy. But these employees were engineers and scientists who had professional opinions about their work and about the organization. The change in policy produced a sharpening of factions, a power struggle, an extensive reorganization, and the resignation of a number of persons. In this series of changes the actors were mainly the scientists and engineers in top staff and line positions. Each man had a set of beliefs about the other persons in the organization. The alignment and conflict of persons holding these beliefs produced a number of changes in the organization which had little to do with the purported aims of the policy change.

The data for this study were collected over a period of two years (1952–1953) of regular visits to the laboratory. Methods of study were for the most part informal: They included interviewing members in private, entering into casual conversations in offices and corridors, attending meetings, and a brief questionnaire.

Formation and Development of the Department

The laboratory was originally composed of three separate groups, two of which were once connected with a university. In 1945, the laboratory

* This chapter is a slightly modified version of "Factionalism and Organizational Change in a Research Laboratory," by Paula Brown and Clovis Shepherd, *Social Problems,* Vol. 34, pp. 235–43 (April, 1956). An earlier version, entitled "The Reaction of Engineers to Organization," was presented as a paper at the annual meeting of the American Sociological Society, September, 1954.

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became one department of a naval research and development station. By 1950, the department contained five divisions: three original groups, and two additional divisions which were first established as service groups to design and build equipment needed by the others. The department had grown to about four hundred members organized into the five divisions, into branches and sections with staff offices attached at both the department and division levels. After the laboratory became a part of the naval station, basic policy and objectives were set by a naval bureau and by the station. The department had to conform to these policies and objectives and to civil service regulations. Within this framework, the department itself determined much of its action.

While the department included clerical and technical personnel, the largest group of its members were civilian professionals. The persons in supervisory positions, of whom we shall speak most, had at least a bachelor's degree in engineering or one of the physical sciences. Another important group of relatively high rank were technical specialists. Among the 175 professionals, 61 percent had bachelor's degrees, 24 percent had master's degrees, and 7 percent had doctor's degrees. The distinction between research and development work and interests was not directly linked with the distinction between science and engineering training, or with that between persons with advanced degrees and persons with bachelor's degrees.

Until 1952, the work done in the laboratory was largely applied research. The higher-level professionals suggested projects which were approved by the naval bureau if they fitted into the research interests of this agency. This resulted in a large number of independent activities. During this time each of the three divisions which did research work was more or less autonomous: divergent technical interests and administrative procedures were pursued, and each division had its own standards and measures of effectiveness. As new division heads took over, they modified the divisions according to criteria which their "engineering judgment" considered most appropriate. These changes included establishing and abolishing branches, initiating projects, and shifting personnel. When a new department head was appointed, there were changes in the departmental structure as well. The department was seen by its members as a collection of individuals and subgroups pursuing a variety of goals and interests. Past associations and friendships cut across some of these ties, but few occasions arose to sharpen factional groupings.

Changes in Organizational Objectives by External Authorities

This pattern of three independent applied research divisions and two service divisions existed for some years. As the Navy clarified and changed its interests, it began to give the department specific requests for equipment development. Because of the kind of work, responsibility for

most of these was given to the service division concerned with development. The entire department became responsible to the naval bureau for a number of large-scale projects. Two important changes were involved: increased cooperation among the divisions in the department, and an increased emphasis upon equipment-development and testing activities. One expression of this was a decrease in the funds available for research and an increase in development funds. In attempting to meet these requirements, department management gradually modified the jobs of individuals and subgroups. Further personnel increases were largely restricted to groups performing the new functions. The cumulative effect of these changes was great, as the groups which became most important in equipment development were the two divisions which had been the core of the department saw these service groups become equal, and then superior, to them (the research divisions) in funds, manpower, and project assignments.

These changes met resistance by many people in the other three divisions. Much of it was phrased in terms of professional values. Such comments as the following were quite common: "It's ridiculous to drop a project just when it's beginning to show results." "I know all there is to know about A and now they want me to drop it and start working on B." "We can't possibly carry out these tests unless the statistical criteria are more specific." "They want us to take on a new job, but no one tells us what to do or when to do it." "They expect us to have the tests completed by September 1, but they haven't given us all the equipment yet."

Some of these objections may be expected to arise in any period of technological change; that is, a man may feel that his competence in a particular area is being ignored, and that he is being required to develop a new skill. Here, the idea that a professional is the best judge of his own progress supported these objections. Furthermore, the professional atmosphere gave a person the right to complain to anyone in the organization. To the extent that the argument was convincing, management might alter its decision. In contrast to industry generally, both supervisor and subordinate were professional engineers. Differences in age and experience were small: 77 percent of the professionals were under forty years old, 53 percent had less than eight years' experience, and 82 percent had less than fifteen years' experience. In many ways, the professional members of the department regarded themselves as a society of equals, any one of whom could exert influence on the others. Thus, they often allowed professional values to override traditional organizational practice.

As the need for coordination between divisions developed, certain staff functions became more important. Some of these staff functions had to be discharged by engineers who were able to set up schedules and analyze results. The position of "project manager" was created for the coordination of the large-scale projects which involved groups from different divisions. This function was not highly regarded. Some typical remarks

by the members of the department were the following: "The staff doesn't realize that they're here only to help the line organization; this expansion of staff is due to overspecialization and civil service red tape." "A project manager is just an errand boy." People who accepted these positions were faced with a personal conflict in that they agreed with the line people that an engineer should work "on equipment" rather than "on paper." But once he became familiar with the requirements, the project manager replied, "They don't appreciate all the detailed work of coordination that a project manager has to do." Each project manager had a time schedule for every phase of his project. Many things could interfere with this schedule, as equipment and manpower were under the control of line supervisors who had time schedules of their own to meet. In an attempt to justify the staff role, and to force others to recognize the significance of the coordination function, the project managers demanded greater status and authority. Since many of them were from the service divisions, such attempts were interpreted by many people as a further wresting of authority from the research divisions. Thus, this attempt to coordinate the work of the divisions was resented by those who felt that they were losing prestige and power.

The Appearance of Factions

As a result of these changes there was confusion, since people could not decide how to divide their time and efforts. Conflicts among individuals and groups for services were common. The department management attempted to meet the change in Navy policy by minor internal changes, such as shifting the work of individuals and groups and transferring some small groups to the service divisions. The ideal of professional independence was being threatened by the demands of the naval bureau and attempts of local management to meet the new requirements. Control of basic policy and specific requirements was now more clearly located outside the department. Many of the older members of the department resented this. The major conflict within the department was between some older leaders and those who, for one reason or another, supported the new policy of emphasis upon equipment development. A number of factional splits appeared, the most striking of which was that between the "old guard" and the new leaders supporting development.

The "old-guard" faction wished to continue research in their field of special interest. The strongest position, taken by a few, was that they were better qualified to establish goals than the "pencil pushers" in Washington. They regarded the new policy as unwarranted interference with scientific work.

The group supporting the change had a choice of several reasons for doing so. Firstly, they could be professionally interested in development rather than in research. Secondly, they could accept the naval bureau's

right to set the policy of the department and their own duty to make the necessary changes. And thirdly, they could expect personal gains in rank, salary, and control of personnel in the new policy.

The leaders of the factions were at approximately the same level in the organization. Most of them were division heads and staff officials. But the equality of formal status did not lead to equal influence or authority. Leaders of the development faction had achieved their status only recently. Their rapid rise was due to a combination of ability, ambition, adaptability, loyalty to the organization, and support of the new organizational objectives. Leaders of the "old-guard" faction had long occupied their positions. Many of them had been in the laboratory since it was established. These "old-guard" leaders had, in the past, often conflicted with one another. They were forced together to oppose the new leaders. But even in coalition, they were overshadowed by the new leaders.

As these factions were developing, the department tried to meet the naval requirements within the existing structure. The department head had a long history of difficulties with the heads of the older divisions. In his attempt to satisfy his superiors, he tended to side with the development faction. In time, the agreement of this faction with the naval bureau allowed the faction to promote a reorganization of the department which destroyed the independence of the divisions and removed the most vociferous of the "old guard" from positions of power.

The Structural Change

The new organizational structure was proposed as a means of delimiting responsibilities and providing coordination for large-scale projects. It was a widespread opinion in the laboratory that the previous organizational structure could have been adapted to the new policy, and that the reorganization added greatly to the adjustments which all persons had to make to the new objectives. Furthermore, the policy change did not require all groups to change their activities. The change in structure was the result of the increased power of the development faction. It introduced a new set of positions to coordinate three divisions. These positions were taken by members of the development faction, who thereby gained control of the majority of the department's personnel. Two other divisions were given the functions of research and testing. The five divisions established in this reorganization were only in part a continuation of the previous five divisions.

The heads of these divisions and the new level of management were appointed by the department head, and the assignment of individuals to these groups proceeded. The composition of some work teams was not changed, although some modification of function was expected. For the majority of persons, however, assignment was in doubt for various reasons. No clear statement of the boundaries between research, development, and testing had been agreed upon. The members of this department

have not been the first to find such definition difficult. But in this case there was another reason for conflict. In order to enhance the scope of his division and attract borderline individuals, each division head used the broadest definition possible. Everyone recognized that these definitions overlapped, but none of the division heads would limit his definition. The greatest conflict was between research and development. A number of persons went into the development group on the promise that they could continue their work, although its classification as development was questionable. The head of the newly established research division found himself with some of his former subordinates, a group previously attached to the department staff, and a few persons from other divisions.

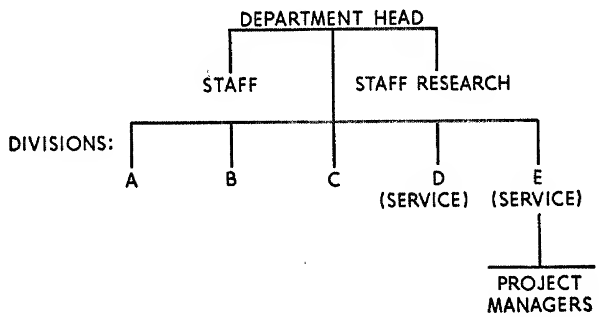


FIG. 1. Organization in 1950.

Among the factors involved in the making of personnel assignments were the following:

1. The need to distribute "experts" throughout the organization; thus when two men were of approximately equal skill in a given field, if one of them chose to go to division R, the other was more or less forced to join division F.

2. The personal desires of division heads for certain men; many promises were made concerning future types of work.

3. Identification of the individuals to be relocated with a faction.

4. Expectations of higher status or increased authority by joining one division rather than another; while no promotions in rank could be made during the organizational change, promises of future promotions were a common way of winning over doubtful persons.

The final allocation of personnel demonstrated even more clearly the triumph of the development faction. Because of the broad definitions used and the changes in organizational goals, the scope of development expanded at the expense of other functions. While future requirements could not accurately be predicted, many people believed that this expansion was greater than that needed to meet the policy change.

The results of the structural changes were (1) to strengthen greatly and enlarge the development group; (2) to give the development group (see Figure 2) a large measure of control over the testing group and its

"old-guard" leader; (3) to set off the research group, under another "old-guard" leader, from the others with independent authority but greatly decreased personnel and funds. Thus the development faction, by its control of funds, personnel, and activities, became the central focus of the department. In 1952, Divisions C and D were combined into testing division (T). The structural changes of 1953 were as follows:

Division A divided between research division (R) and new development division (F).

B and E continued with few changes.

Staff research joined research (R).

Some members of T joined research (R).

New level of management, development office, created and included project managers.

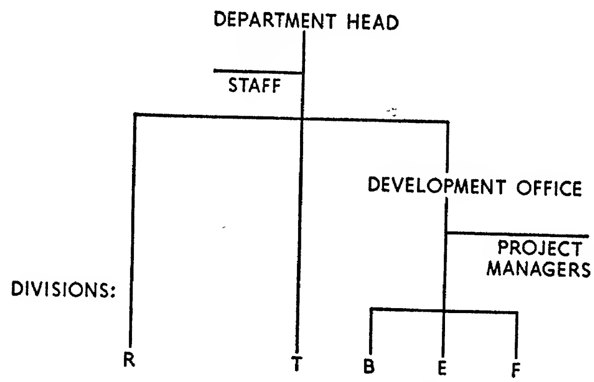


FIG. 2. Organization in 1953.

The new power situation was so strikingly different from that which had held in the past that the leaders and some members of defeated factions left the organization. They gave as their reasons for leaving that a vital function (theirs) was being disregarded, or that their groups could not properly perform their work without certain facilities (testing site, equipment, etc.) which were now under the direction of others. Other persons left during or after the reorganization period because they did not want to go through the extensive readjustments necessary. A number of these people had been considering other positions, and they took this occasion to leave. A few of the men who left did so because they felt that the new policy did not allow them to do the work in which they were interested. Still others left because they did not wish to engage in the factional struggle.

Analysis of Factions

The issues involved in this example of factionalism may be classified into three categories: objectives, in-group loyalties, and personal relation-

ships. There was a pair of problems connected with the first category. One was the acceptance of the naval bureau's and station's right to set or modify objectives. Many of those who participated in the development of the original organizational objectives and in later modifications regarded themselves as the scientific experts best qualified to plan the department's program. The assertion of this authority by the naval bureau and station management was regarded as an unwanted intrusion in their realm of influence. Other members of the department accepted the naval bureau's authority to make these decisions and attempted to meet the modified objectives. Some of these people supported the new objectives because they thought that development was more important than research. Thus, there were two sources of disagreement: (1) who should set objectives; (2) which objectives were best. Either of these questions could bring individuals into a faction. Eight months after the structural change took place, 58 percent of the department members felt that the department *should* be devoted to development rather than research, and 71 percent of the department members felt that the department *would* emphasize development rather than research. Compared with those who chose research as the goal which the organization should pursue, those who preferred development were more confident of their ability to handle their positions and were interested in higher positions within the organization.

The second problem connected with objectives is that of the personal goals of the members of the department. In responding to a questionnaire each professional answered this question: "If you could achieve a wide reputation for just one thing, would you prefer to be known for: (*a*) a general research idea; (*b*) being a good fellow to work with; (*c*) developing useful equipment; (*d*) an original formula; (*e*) organizing the work of a successful group; or (*f*) applying a known principle to a new and important use?" It is interesting to compare these goals of individuals to their reference groups. Persons with reference groups outside the station tended to have pure and applied research goals (*a*, *d*, and *f*). With both reference groups and personal goals located outside the organization and its primary aims, these persons could hardly be expected to ally themselves with current organizational policy. In contrast, a significant proportion of persons whose reference group was within the station had supervisory goals (*e*), and those with their occupation as a reference group tended to have development goals (*c*). Persons in these last two groups might be expected to identify with the organization and its current objectives, but for different reasons. Personal goals also varied with position: the lower levels of professionals had more pure research goals (*a* and *d*); middle levels had more applied research and development goals (*c* and *f*). The interpersonal goal (*b*) was chosen by only 4 percent of the professionals.

The second category of issues arising from factionalism is that of

ingroup loyalty. In the laboratory, work teams attempted to protect their membership and functions from the inroads of others. This was seen in the conflicts which arose about the allocation of responsibility and personnel for specific projects. It occasionally took the form of failure to cooperate with other groups. Communication between work teams was often difficult and sometimes broke down completely. On the whole, work teams were kept intact when structural changes were made, because of a feeling that the team members should remain together if possible. Some time before the reorganization described here occurred, two divisions were combined. However, no attempt was made to redistribute members; rather, the component branches remained separate. There was some antagonism between the component groups. Later, at the time of the major structural change one division was divided between two of the newly created divisions. Many supervisors objected to the breaking up of their groups, and when possible, the sections of a branch were kept intact in the transfer. In theory, the reorganization should have involved a similar segmentation of another division, but ingroup loyalty and compromises made in the definition of functions allowed the entire division to be included in the development group. When a supervisor transferred to another division, many of his subordinates accompanied him. The branch heads, though interested, did not all take strong positions in the dispute, and many tried to keep their personnel out of the struggle. One branch head told us, "I held a meeting in my branch and explained the reorganization as well as I could. I suggested that they talk to anyone they like about it, and decide what they wanted to do. Then when I had another meeting some weeks later, none of them had decided. They asked me what I wanted them to do, while I had tried to get them to decide for themselves."

The third category of issues is that of personal relationships. Many people found in the department, but not in their work groups, former schoolmates, neighbors, bridge partners, car-pool members, fellow members of clubs, sportsmen, etc. By their participation in these groups some people were influenced in their choice of assignment. Our analysis of observational records, interviews, and sociometric responses shows that this participation was sometimes a decisive factor.

Hence, each person was influenced by his attitude toward the objectives of the organization, by his group loyalties, and by his personal relationships. Some persons had little difficulty in deciding which group they preferred. For others, their affiliations resulted in conflict. Their final decision was based upon the superior strength of certainties. For example, one section head preferred research work and had ties with others in research, but joined development because of the pressure applied by higher management and because of his friendship with a branch head in the development group.

The factional split was based upon clash of attitudes and beliefs of

different kinds. Some of the attitudes and beliefs were probably acquired in the professional schools. Still others developed from the separate interests of the five original divisions. While diverse attitudes about the purpose of the organization, or of parts of it, could exist as long as the parts remained somewhat independent, the need for a unified organization brought these differences into conflict. In the course of the conflict, new leaders, who were not loyal to the "old guard" or to the old way of doing things, could succeed. If there had been in the department a general willingness to accept the new objectives and modify behavior accordingly, the structural changes might have been unnecessary. Authority could have been more equitably distributed among the former leaders. In addition to the desired effect of bringing about greater coordination, a job first attempted by the project managers, the reorganization had the further effect of making many individuals so uncomfortable in their new positions that they left. Some persons who increased their status as a result of the change left within the year to accept positions in other organizations which promised less strain and conflict.

Conclusion

The process of change in this laboratory has been shown in three phases:

1. From 1946 to 1952 the department was a relatively stable group in which divergent objectives and activities were pursued.
2. The imposition, from outside, of new demands on the organization required the modification of organizational objectives and the increased coordination of activities. At first, minor changes were made. However, the modification of objectives and the rising importance of different functions and activities threatened the power and independence of older groups. These changes were resisted by the older groups for a variety of reasons.
3. A structural reorganization, proposed as a more efficient way of achieving the new objectives, resulted in greatly increased power for one faction, and the resignation of some former leaders.

The differences of objectives within the organization were of minor importance while the subgroups were relatively autonomous. But as the organizational requirements changed, these differences became the basis of sharply conflicting factions. A faction composed of leaders of former service groups rose to power. This faction, in agreement with external authority, became strong enough to promote a structural reorganization and thereby further increase its power. The "old-guard" group had to change their activities to some extent. But their greatest loss, and the one to which they objected most, was their loss in power and independence. Thus their professional interests were combined with their feelings about the organization and their positions in it.

The official explanation for the reorganization was that it would better meet the current objectives. This explanation was accepted by the station and by the naval bureau. But an analysis of events and attitudes within the organization suggests that the modification of objectives and need for coordination were conditions which brought the underlying differences into conflict. The factions, rather than the policy, appeared to produce the organizational change.

SECTION III

Issue Cases

Chapter 13

*INDUSTRIAL CHEMICALS, INC.**

INTRODUCTION

INDUSTRIAL CHEMICALS, INC.,[1] was, in the spring of 1954, a chemical specialty manufacturing organization with an annual sales volume which, in 1953, had been close to $70 million. The company's plant and offices were located in the heart of a large middle western city and were housed in a sizable modern building which had been erected in 1946, to consolidate the entire operation in one place. Since occupying the new building, the company had continued to expand and there were frequent additions made to the original structure.

Originally established before 1900, Industrial Chemicals had been founded to manufacture and sell chemical compounds with a number of end uses. Generally speaking, these chemicals were sold to manufacturers and were used in the processing of industrial products. Through the years, particularly after synthetic chemicals came into general use, the management of the company undertook production of any chemical for which there was a reasonable demand and which could be produced profitably in their plant. By 1954, this policy had involved them to a considerable extent in various retail markets as contrasted with bulk sales to industrial customers. Retail products included a household detergent, a patented organic fertilizer, an improved insecticide, and a line of dyestuffs packaged for use in the home.

In most cases, Industrial Chemicals bought the rights to their early synthetic compounds from the European laboratories where they had been discovered. In later years, they supported research in American universities and finally, in the early 1930's, they initiated a modest research effort of their own by hiring an organic chemist and equipping a small laboratory for him in an old warehouse. At that time, the manufacturing operations were carried out in an old building more than a mile from this warehouse, and a number of executive offices were at a third location not far from the plant.

The organic chemist, Dr. Kurt, worked for several years on projects of

* Copyright, 1954, by the President and Fellows of Harvard College.

[1] All names and certain other factual data have been disguised to conceal identities.

his own choosing. He synthesized a number of new compounds, but none of these were judged by company management to be of commercial value. Eventually, however, he succeeded in making a compound which proved to be the ideal solution to a universal problem in the processing of certain high-volume consumer goods. After an extensive period of development, this new compound was placed on the market. It was a tremendous success, and on the strength of sales and profits of this one compound, the company's heretofore modest growth curve spurted upward. Whereas sales volume in 1935 had been approximately $10 million, this had increased to $25 million by 1942, a year in which sales of Kurt's compound accounted for more than half of the total volume.

Impressed with this dramatic demonstration of the fruits of research, management decided to build a research and development organization which, they hoped, would provide the ideas for additional high-volume products. The first steps were modest ones. A second organic chemist. Dr. Hyler, was hired to work with Dr. Kurt, and Dr. Myers was brought into the laboratory to work on development problems. As sales increased, the laboratory was expanded until, by the mid-1940's, it had outgrown the available space in the old warehouse and was overflowing into adjoining buildings. At the same time, the expansion of sales volume had put an impossible burden on the existing plant facilities. Since profits earned by Kurt's compound had placed Industrial Chemicals in a strong financial position and since the potential of several development projects indicated a further jump in sales volume, management decided to go ahead with plans for a new building. Although the war delayed the execution of this plan, the building was finally occupied late in 1948.

The building, when completed, was a four-story structure occupying an entire city block. It was located near a shopping and business center in a pleasant semi-industrial section. The basement and first two floors were largely occupied by the manufacturing division, but a large modern employee cafeteria was located on the ground floor and the executive and sales offices occupied part of the second floor. The offices and laboratories of the research and development division were on the third and fourth floors.

The decor of the entire building emphasized modern efficiency but was not in any sense luxurious. Every desk in the building, with few exceptions, was a standard metal product which was attractive and efficient but unpretentious. Relatively few executives had offices as such, and even these usually had glass walls. Floors throughout the building were covered in asphalt tile, and there were a number of large general offices which contained the desks of a dozen or more people.

The various individual and group laboratories of the research and development division were large, well-lighted, and completely equipped. Executives' and supervisors' offices were located close to the laboratories they supervised and a number of plainly furnished conference rooms were

placed at convenient points on the third and fourth floors. The actual layout of offices, conference rooms, and laboratories changed constantly between 1948 and 1954 as the research and development division expanded and went through several major organizational changes.

The research team from the Harvard Business School began observing the activities of the research and development division early in the spring of 1954. During the first few weeks of the research, the two men made a number of general observations which helped set the stage for the things they saw and heard later. These observations may be grouped into four main categories: the competitive situation; organization of the research and development division; research and development management policies; and finally, atmosphere and employee morale.

The Competitive Situation

In an industry dominated by a number of very large companies, Industrial Chemicals had survived and prospered by marketing products that were distinguished by one or two features that set them apart from those marketed by their competitors. Although a small part of their business was still done in bulk chemicals, they had gradually turned more and more toward retail markets, with attendant higher margins and less chance that the giants of the industry would force them out of business by using their high-volume operations as a price lever. Typically, their product lines were initiated by a sales-oriented management that was able to spot opportunities, and implemented by a competent development organization that was able to meet the demands of the market by coming up with ideas that enabled Industrial Chemicals to introduce diversified product lines which were, in many instances, a little better in some ways than anything being produced by competing companies.

For instance, a number of their products were chemicals that were: (*a*) put up in unusually efficient packages, (*b*) had one unusual property which had been gained by adding other chemicals to the standard formula.

It will be noted that, with a few exceptions, notably the compound synthesized 20 years before by Dr. Kurt, these products had originated with top management and/or the scientists of the development laboratories. In only eight cases had completely new compounds synthesized or otherwise discovered by the research laboratories gone through final development and reached the market. Perhaps a score more of the thousands of compounds in the research laboratory files had definite market potential, but for one reason or another management had decided not to produce these chemicals.

The product lines of Industrial Chemicals were usually short-lived. Typically, a new product would have relatively heavy sales for a few years and then gradually decline to a moderate or low level. As a result,

management was constantly on the lookout for new product ideas and depended heavily on the development organization for the speedy implementation of these ideas. Although provided with a substantial base supplied by the steady high-volume sales of Dr. Kurt's compound, it was evident to management that further expansion depended on a steady stream of new products.

In deciding which products to market, management typically looked for "holes in the dam." If the market for a potential product, even though it were a somewhat improved version, was dominated by one company with a well-established name in the field, ICI management usually decided not to incur the heavy expenses of trying to promote their version. If, on the other hand, they had a clearly superior product in an area where there was no outstanding leader, they would be inclined to promote the product heavily. There were, of course, many marketing decisions that had to be made involving competitive situations that were not as clear-cut as the examples cited above.

Generally speaking, ICI produced chemical products for markets where high margins were the rule and where competition was based more on quality and proven performance than on price. Manufacturing costs were generally low, sales and promotion costs proportionally higher than in many industries and research and development costs, budgeted at $2 million in 1954 on projected sales of $70 million, represented a substantially higher expense as a percentage of sales than reported by most industries.[2]

The net effect of the competitive situation on the research and development division of ICI, as observed by the research team, was that pressures exerted on division executives by top management to produce new products were constant. Pressures emanated from the same source, although more sporadically, to keep research and development costs down, particularly when general business conditions were depressed.

Organization of the Research and Development Division

The formal organization of the research and development division is outlined in the organization chart (Exhibit 1). It will be noted that Dr. Mace, the director of the research and development division, reported directly to Mr. Shaver, the president of ICI. He was assisted by Mr. Wright, who, as business manager, supervised a variety of administrative functions including work on budgets, personnel, patent applications, and project coordination and who supervised directly the sizable library and service groups.

The second echelon of management was made up of two men, Dr. Hyler, the director of the research laboratories and Dr. Myers, the director of the development laboratories. Both of these men had been with

[2] See DeWitt C. Dearborn, *et al.*, *Spending for Industrial Research, 1951–1952* (Boston: Division of Research, Harvard Business School, 1953).

ICI for 20 years in 1954 and, together with Dr. Kurt, had been the original trio of scientists hired in the very early days of the research and development history of the company. Dr. Mace, on the other hand, had been appointed director of the division in 1950. He had previously spent a number of years on the faculty of a leading university and previous to his appointment at ICI had held an executive position in the laboratory management of another company.

Dr. Kurt, who appears on the organization chart as senior scientific advisor, had no supervisory responsibility. As a result of his important discovery in the 1930's, he had been handsomely rewarded financially and, when the company moved into the new building in 1946, had been given a completely equipped laboratory adjoining those of the other organic chemists. He was free to work on whatever interested him whenever he chose to do so. Since his income from company securities had made him independently wealthy, he spent a number of months each year traveling. When he was working, he often collaborated informally with his younger colleagues if their work interested him, and it seemed to the observers that he was regarded with respect and affection by nearly everyone who worked for the company. Although he was over 60, he was invariably addressed or referred to by his nickname. Everyone, from Mr. Shaver down to the lab technicians just out of high school, called him "Rod."

By 1954, there were four departments within the research laboratories, each supervised by a department head who reported to Dr. Hyler. Each department head was responsible for the research projects budgeted to his department. This system was complicated by the fact that personnel of, say, the organic chemistry department often worked on research projects budgeted to other departments or, not infrequently, development projects that were financed with development funds. The series of cross-charges that resulted from this situation was handled by Mr. Wright's staff.

Research programs originated in several ways; in most instances, a project developed out of interest expressed by one of the research scientists in a particular field or area. Often, a scientist would do preliminary work on a new idea during *elective* time and, if results were encouraging, then try to interest his supervisors and colleagues in his ideas or leads. If he succeeded in doing this, the idea would then be presented to the research committee together with a request for financial support. If the committee reacted favorably, the idea would become a part of a formal project and the interested scientist would usually be assigned to work on it.

Projects were also on occasion initiated by suggestions from someone in the management group or as the result of suggestions made by outside consultants in various fields. Assignment to these projects was normally put up to the scientists as a matter of choice—that is, a man would not be asked to work on something that did not interest him.

In nearly every instance a research department would have a broad

major project in its particular functional field which would be staffed by a group of scientists, and would also be working on a number of subsidiary projects each staffed by a senior scientist together with his juniors and technicians.

The chemical processing department was unlike the other four research departments in that, as its name implies, it was not devoted to research as such, but rather to servicing the other research departments. For instance, if an organic department scientist synthesized a new compound which looked particularly interesting, the chemical processing department would be asked to make a larger batch of the compound for testing purposes. This would then be turned over to the analytical department for testing or perhaps to a development project group.

The development laboratories under Dr. Myers were organized somewhat differently from the research laboratories. Reporting to Dr. Myers were two associate directors, Dr. Rossian and Dr. Holden. Dr. Rossian was directly responsible for the development project groups, which handled most of the development work, and for a small product development laboratory. Dr. Holden, besides having supervision over the pilot plant, was in charge of new product development which was centered in a special group called Special Project X. The function of this group was to look for and do preliminary development work on new ideas for products. If this preliminary work convinced management that the idea had possibilities, further development of it was carried out by a project group under Dr. Rossian.

Interlaced with and vital to the operation of the line organization of the research and development division were a number of committees. As a matter of policy, much of the planning was done as the result of committee recommendations and practically all important questions were referred to one or another or several of the committees for decision. Dr. Hyler, as chairman of the research committee, had the responsibility of making decisions involving research activities or of referring questions to the research and development operations committee, chairmanned by Dr. Mace. Dr. Myers, as chairman of the development committee had a similar responsibility in his area. Dr. Mace sat as a member of both of these committees. Mr. Shaver, the president, was a member of the research and development operations committees and of the new products committee. The latter body, which was also headed by Dr. Mace, considered questions involving the new ideas developed by Dr. Holden and his group.

Research and Development Management Policies

In an early conversation with the two researchers from the Harvard Business School, Dr. Mace outlined a number of policies which he had developed and which constituted his management philosophy. He stated these policies as follows:

1. *Function of Committees.* Decisions referred to committees in the research and development division are to be made by the chairmen of the various committees. The other members of the committee act as advisors to the chairman to help him reach decisions which make sense.

2. *Right of Appeal.* The preceding policy carries with it a policy that everyone should be content with a decision that has been made. If an individual or group should have serious reservations about the wisdom of a decision, it becomes the responsibility of the committee chairman to review it. If he cannot satisfy the dissenter or dissenters, the decision should be modified or further study should be undertaken of the facts involved.

3. *Information.* Personnel down to the lowest echelons are to be kept fully informed of what is going on in the division, even on confidential matters where the danger of leakage to competitors is substantial. Minutes of meetings are distributed widely throughout the division as are certain reports and memoranda.

4. *Channels.* Information may be passed back and forth outside of formal channels whenever it seems desirable, and it is not necessary to keep intervening echelons aware of such information unless it is obviously appropriate. Decisions, on the other hand, must go through channels, or if decisions are made outside of channels, intervening echelons must be kept informed of decisions that have been made.

5. *Cooperation.* While not a formal policy, Dr. Mace regards the preservation of cooperative attitudes as his responsibility. He believes a spirit of cooperation was part of the organization when he came into it and he guards it carefully as the chief executive of the research and development division. He sees one manifestation of this cooperative spirit being the idea that subordinates work with, not for, their superiors. He guards the spirit of cooperation by insisting that no one "builds fences around his organization" and by hiring people who show ability to get along with others rather than those who have extraordinary technical skill, but who cannot or will not cooperate with their colleagues.

Atmosphere and Employee Morale

Although there were numerous individual complaints concerning work and relationships within the research and development division, the researchers from the Harvard Business School reached a conclusion early in the period of their research at ICI which they found no reason to change during later stages of the research. They quickly sensed that employee morale in the research and development division was very high and evidence to support this belief far outweighed evidence to the contrary in the weeks of observing that followed.

In trying to account for this high morale, the researchers noted a number of facts and opinions. For instance:

1. *Physical facilities,* such as work places, rest rooms, laboratories, recreation rooms, and the employee cafeteria were all of the highest quality. The cafeteria particularly seemed unusually good in regard to the quality and planning of menus and the low prices charged.

2. *A spirit of informality* was in evidence from the president of the organization on down. Mr. Shaver habitually ate in the cafeteria or attended committee meetings in his shirt sleeves, and it was apparent to the observers that everyone

else dressed as they pleased. Secretaries smoked at their desks or while taking minutes at committee meetings. Everyone called everyone else by their first names.

3. There was a pervading *atmosphere of confidence* in the integrity and intelligence of the individual employee which manifested itself in several ways. One of these was the handling of "coffee breaks." The rule, well known to everyone, was simple—"you may take a coffee break between 10:00 and 10:30 in the morning and another between 3:00 and 3:30 in the afternoon. Please do not stay away more than 15 minutes and please do not abuse this privilege." It seemed obvious to the observers that no one did abuse the privilege. Another example was the lengths to which top management would go in keeping people informed. As one junior executive put it, "I can get the quarterly profit and loss figures from the accountants before the board of directors see them—all I have to do is ask." A group of junior scientists stated, when questioned by one of the observers, that they felt a sense of belonging and of being trusted. They said they were regarded by management as adult human beings who wanted to do a good day's work instead of as children who would get away with anything they could.

4. The observers also noted a *spirit of helpfulness* that appeared to be nearly universal. People helped each other in a number of ways. One way was what the observers came to call "the gentle needle." This consisted of one employee quietly or jokingly calling to the attention of another something left undone or a report that required action immediately. This spirit was also evidenced in the way everyone in the organization went out of his way to steer visitors correctly or to indoctrinate new employees.

5. Finally, *financial rewards* were generally regarded as better than average for the industry and type of work being done.

A Word about the Cases

The rest of the story of the research and development division of Industrial Chemicals is told in the series of cases that follows this introduction. Each case represents a distillation from the notes of the observers—it therefore is readily apparent that individual cases do not represent a complete picture of what went on at a particular place or time, nor, in the broader sense, does the series as a whole tell the complete story of the research and development division. Each case does contain a report of those observations that the observer happened to make, the words he happened to hear or the feelings he thought were being expressed by the people he was observing at a particular time and place. The series as a whole contains perhaps 2 percent of all the observations made by two men during the period of research at this company—the element of selection is therefore doubly obvious in the material that has been finally included in the cases.

In selecting the material as we have, we have tried to accomplish several objectives:

1. To include individual cases which are truly representative of the total situation at ICI and, at the same time, are subject to analysis in reference to the problems of administering research organizations as they were stated to us by directors of research.

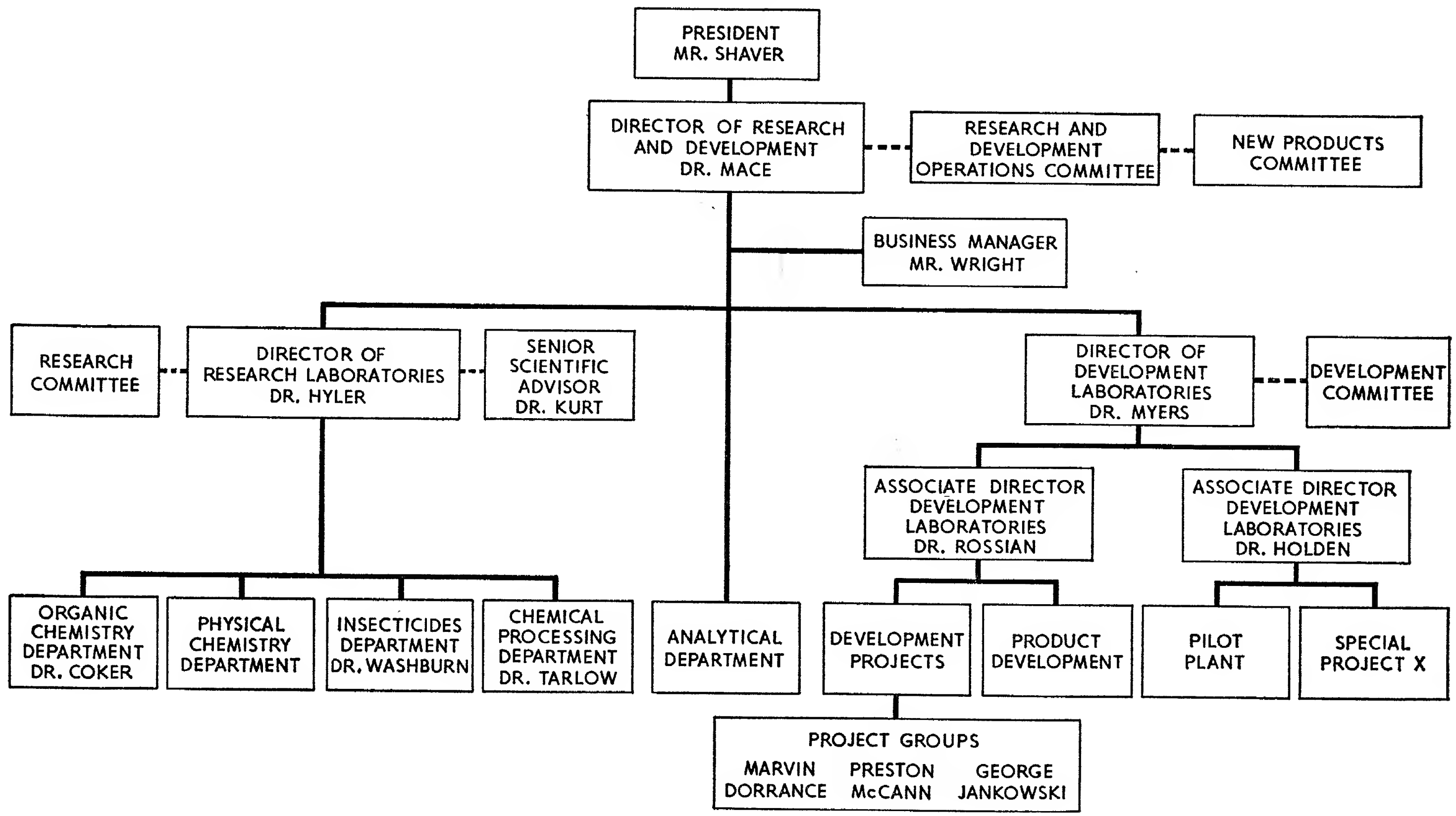
2. To report faithfully those parts of our material which we believed to have been least influenced by our own presence on the scene.
3. To tie together a series of incidents, opinions, attitudes and related observations which would give the reader a good "feel" of the organization concerned.

We do not believe that the story told by these cases is, in any sense, typical. No more so, certainly, than would be a similar story told about any other industrial research organization. We have here a particular group of people wrestling with the problems of administering the research organization of a particular company which is, in turn, trying to maintain its competitive standing in a particular industry—all this at a particular time in the dynamic history of the people, the company and the industry. In spite of this imposing bill of particulars, we might expect that the problems and the methods of dealing with them reported here are not unusual—are, in fact, readily recognizable to anyone who has ever worked in an organization devoted to industrial research.

Exhibit 1

INDUSTRIAL CHEMICALS, INCORPORATED

ORGANIZATION CHART R & D DIVISION



Chapter 13 (Continued)

INDUSTRIAL CHEMICALS, INC. (A)*

IN THE SPRING of 1952, the director of research and development and the business manager of the research and development division of Industrial Chemicals, Inc.,[1] were faced with the task of finding a suitable man for the job of department head in the organic chemistry department of their research laboratory. The former department head, Dr. Hyler, had been promoted to the job of director of research laboratories. He had continued, in addition to his new duties, to function as department head of organic chemistry, but this arrangement had not worked out well, and Dr. Hyler had pointed out that there was an evident need for a man of supervisory stature who could take a good deal of administrative burden from his shoulders.

Selecting this man proved to be difficult. Dr. Hyler had selected as his senior scientists a number of brilliant young research chemists. Mr. Wright, the business manager, and Dr. Mace, the director of research and development, were agreed that, unfortunately, he had not only selected men of decided academic leanings but had not been successful in his efforts to develop one of these men as a potential replacement for himself. Since the two men believed that any supervisor in the research organization of Industrial Chemicals must be an extrovert, able to sell research programs to the top-management group, they found themselves unable to agree on a likely candidate for promotion from within the organic chemistry department.

Industrial Chemicals, Inc., spent about $2 million annually on research and development. There were two development laboratories in the research and development division and five smaller laboratories devoted to research. The research effort was supported by about a quarter of the total R & D budget.

Mr. Shaver, the president of the company, was familiar with and close to the development activities of the laboratories. Through the years, he had contributed many worthwhile ideas that led to the development of successful products.

There were a number of committees set up in the research and

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[1] All names have been disguised.

development division that played an important part in the making of decisions and in transmitting information throughout the division. Mr. Shaver was a member of the new products committee, which had been organized mainly for the purpose of keeping him up to date on development activities and decisions, and of the research and development operating committees. Members of these committees had repeatedly expressed their feelings about Mr. Shaver's participation in the meetings in terms like these:

> Mr. Shaver meets people outside the company all the time and gets bright ideas which he brings up in committee meetings. Often the people he gets the ideas from are sales oriented rather than scientifically oriented. Usually, we have thought of the idea and discarded it. But you have to take him seriously because he is really interested in development work and is usually very helpful to us. It would be a real loss if he stopped being interested in our work just because we arbitrarily discarded his ideas without giving them serious consideration.
>
> Mr. Shaver is a very colorful, energetic person who every once in awhile gets concerned. Usually he gets concerned because a product is giving us trouble from a marketing standpoint and when that happens he gets excited and puts on the pressure.
>
> Mr. Shaver's questions at meetings are such that he is usually putting pressure on somebody. This is okay as far as development goes—his questions keep people on their toes. But when he questions the research people it causes trouble, because, even though he tries hard to see things from their point of view, he just doesn't understand research and his questions naturally get people upset at times.

When the research committee had been formed in 1950, Mr. Shaver had attended several meetings. Although it seemed clear to the scientists on the committee that Mr. Shaver's technical knowledge was limited, he often tried to influence decisions which the scientists believed should be decided on the basis of the very technical facts which he did not understand. In one way or another they soon let Mr. Shaver know how they felt about this and, believing that he had nothing to contribute to these meetings, Mr. Shaver ceased attending them.

The president's attitude toward the research effort, Mr. Wright believed, was partially influenced by the fact that, in the late 1930's, the man who was then the company's lone organic chemist had synthesized a new compound which eventually accounted for more than a third of the company's total sales. It was largely the profits earned by this product which had financed the greatly expanded research effort of Industrial Chemicals. But one result of this, according to Mr. Wright, was that Mr. Shaver now felt that the research organization was not keeping pace with the competition in the race to discover new high-volume products. His recurring question, when advised that the director of research laboratories was recommending a new field for research would be, "Will this area give us a salable group of products?" When Dr. Hyler had to say that he could not promise this, Mr. Shaver would reply, "Then why do you want to spend money in that area?"

Although Mr. Shaver, in the spring of 1952, was no longer attending research committee meetings, he had, in the last analysis, the final word of approval on the research budget. Therefore, in an effort to keep informed regarding the status of research programs, he formed the habit of calling into his office Dr. Mace, or any other man in a supervisory position in the research organization, whenever he wanted information on a research program. It was for this reason that Dr. Mace and Mr. Wright were anxious to have as department head of organic chemistry a man who could perform well if called in by Mr. Shaver to explain the work being done in his department.

Having decided that none of the men then in the organic chemistry department could perform effectively as department head, Dr. Mace and Mr. Wright came to the conclusion that a man would have to be brought in from outside the company. This conclusion was influenced by two factors: first, they knew that Dr. Hyler had recently experimented with rotating three of the more likely seniors into the department head job on a six months' trial basis. None of them had shown sufficient ability to relieve Dr. Hyler of administrative tasks, and without exception they seemed unable to speak effectively about their work when called in by research executives. Second, their experience had been that Mr. Shaver was far more inclined to listen respectfully to outside consultants or university professors than to the men in the research organization of Industrial Chemicals.

Between June of 1952 and March of the following year, therefore, the two executives considered the qualifications of a number of possible candidates for the job. They actually offered the position to two eminent chemists, both university professors whose names had been suggested to them by Dr. Hyler, but both men turned the opportunity down, saying they would rather stay in academic life.

When the second man made known his negative decision, it seemed to Mace and Wright that the only alternative lay in a reconsideration of their decison not to promote one of the seniors in the department.

Together with Dr. Hyler, they reviewed again the qualities of the men in the section and decided that Wayne Coker, a PH.D. who had been with the organization for ten years, seemed to have the highest potential. Coker seemed to be respected by the other seniors, had an excellent mind, and Dr. Hyler believed he could work into the job very well. In fact, Dr. Hyler found it difficult to understand why his superiors were so anxious to have an "extrovert" in the job. As he saw it, a department head should first of all be a good scientist, whose scientific opinions and ideas were respected by his colleagues. Second, he should have the ability to handle administrative details and good judgment in editing reports, papers, and memoranda written by the men under him. Finally, he should know the company well so as to be able to provide the services for his men they needed to do their work. In his estimation, Wayne Coker had or could

develop all of these qualities and he saw no reason for asking him to be a salesman as well.

Confident that Coker would measure up to his new responsibilities, Dr. Hyler suggested that he originally be made assistant department head and only be promoted to department head if, at the end of six months' trial, he had demonstrated the required qualities. The other two men agreed to this plan and Wayne Coker was promoted to assistant department head of the organic chemistry department on June 1, 1953.

On January 15, 1954, after telling Dr. Mace that he intended to do so, Dr. Hyler wrote a memo to Dr. Mace, suggesting that Coker be promoted to department head. In the memo, he stated that Coker had done an excellent job of taking a large part of the administrative burden from his shoulders and that it seemed clear to him that Coker would continue to perform well as department head. Several days after he received this memo, Dr. Mace asked Dr. Hyler to come to his office to discuss the matter. Dick Payton, a researcher from the Harvard Business School, was present during the ensuing conversation and took down notes from which he reconstructed a largely verbatim transcript as follows:

DR. MACE: Jack, I asked you to come down because I wanted to talk over with you your memorandum suggesting that Wayne Coker be promoted to department head of the organic chemistry department. I've talked to a number of people about this and I want you to know that all of these men agree he is doing a good job. He has certainly taken hold very well, and there is no question of the fact that his potential is very good indeed. However, there is one thing that is worrying all of us about Wayne and that is his tendency to stay in a shell and not to express himself. He just doesn't speak up in meetings or in personal contact with people in the administrative group, and we believe that we ought to speak to him about this before going ahead and making the promotion to department head. Probably the best way to handle this is for you to speak to him first. We are all pretty worried about this tendency on his part. I've noticed it particularly in him at the cracker barrel meeting[1] the other night. He didn't talk at all at that meeting.

DR. HYLER: Wayne is quiet by nature. He doesn't like to talk just to talk. He likes to consider what he is going to say, and when he says something, he really has a point to put across.

DR. MACE: I recognize that. But he just doesn't make a very good impression. The other day when I had him down at the office to talk about how things had been going in the past six months, I had to carry the conversation almost entirely by myself. I would ask Wayne a question and

[1] The scientists of Industrial Chemicals, Inc., met occasionally in the evenings for dinner and informal, off-the-record discussion of matters of mutual interest—usually new ideas for the research and development division. They had come to refer to these gatherings as the "cracker barrel."

he would answer "yes" or "no," or with very short statements. This kind of behavior makes people uncomfortable.

DR. HYLER: Well, we've talked about this before, but I thought that Wayne had improved a lot. You remember we talked about this when we made him assistant department head, and remember too that it was my suggestion that we make him an assistant department head instead of department head so that we would have a chance to see how he would develop. It has been my impression that he has developed very well. What does this all mean anyway?

DR. MACE: You see, when we make this promotion to department head the step is irrevocable. I think what we are all saying is that we want to see if he can come out of his shell before we take that step. When he gets to that position, a lot of demands are going to be made on him to explain the work of his department at meetings and elsewhere. People will think he is a dumb bunny if he never says anything.

DR. HYLER: Well, I guess I can go ahead and talk to him. Let's decide how I can approach this in the best way.

DR. MACE: Well, as I said, I think the initial conversation should be with you, but I will be glad to talk to him after you have talked to him. I think that what you want to do is to emphasize the importance of speaking up and get him to understand that he's got to talk in these meetings and say what he thinks.

DR. HYLER: I've talked to Wayne some about this. We knew of his inherent characteristics when we made him assistant department head.

DR. MACE: At the time we made him assistant department head we felt he was the one in the department with the most potential. That is why we made him the assistant department head. But before we take this further step we feel that we should talk to him about this one difficulty.

DR. HYLER: I think giving Wayne more responsibility has worked out very well. The atmosphere in this department is much better and he has really taken hold and done a wonderful job. In discussing this with Wayne, will it be all right if I tell him that it is not from within the department that this comment has been coming but that outside the department there are people who want him to talk more?

DR. MACE: Sure, that's it. You can certainly say that.

DR. HYLER: Well, are we going to leave Wayne with the impression that his promotion to department head is dependent on his coming out of his shell?

DR. MACE: Well, it is not completely dependent, but it is certainly very desirable. Of course, he doesn't know about your memo, so actually the promotion to department head doesn't have to come into it.

DR. HYLER: No, he doesn't know about the memo, but he does know that I have been meaning to make the suggestion that he be promoted. I've asked him before if this is the way he wants to go, and he has said yes, and I told him that I was very pleased with his work so that he knows that I

want to elevate him. I don't want to give him the impression that there is a reluctance about that.

DR. MACE: No, don't do that. Just say one other thing has come to your attention and that is this thing about speaking up at meetings; talking more to people about the work of his group. As you know, this is very important around here with all the meetings we have. The further up you go, the more important it is. I tell you I was mighty uncomfortable when I had him down here and other people in the administrative group feel the same way.

DR. HYLER: Don't you think if we gave him the responsibility—.

DR. MACE: If we did and he didn't come out of his shell and start to convince people that he knew his stuff, we'd be in the soup.

DR. HYLER: I agree with you, but I can't afford to tell Wayne that he can't go any further.

DR. MACE: He is still in the position where you can work on changing this one characteristic. If we made him a department head, and he still didn't talk, it would put us all on the defensive.

DR. HYLER: I don't have any hesitancy about his ability to do a good job.

DR. MACE: Well, now Jack, Peter White [*one of the seniors who had been rotated into department head job for six months' trial*] was an example of what I have been talking about. That was a very unfortunate situation. You remember when Ralph Myers [*director of development laboratories*] asked him one time to come down to his office and talk about the polymer program, and all that happened was that Ralph would ask him questions and Peter would say "yes" or "no" and make some short statements, and it all ended up with Ralph wondering if Peter knew what he was talking about.

DR. HYLER: I must defend Peter. That was a difficult time to convince Ralph Myers of anything, particularly about the polymer program.

DR. MACE: Once Coker is department head, he could be called down any time by Ralph Myers, or even Mr. Shaver, and once a negative impression is set up, it is pretty hard to reverse it. I could deal with it better because I am used to dealing with different types of scientific personnel, and I know that this is the way that many of them are. But other people in this organization are just not used to that sort of thing. They expect people to talk up and say what they feel.

DR. HYLER: The thing that really bothers me is the fact that we went through this for a long time and talked about it a lot when we were making the decision to make him assistant head and our final decision was that he was a good bet.

DR. MACE: Sure, he had the best potential, and we wanted to give him a challenge and see if he could meet it, and he has met it in every way except developing his personality in this one way.

DR. HYLER: You are saying then that he has met this challenge except

in one way and if he can't meet this further challenge, there will be some doubt of his promotion.

DR. MACE: Yes, that is exactly what I mean.

DR. HYLER: I think we will just have to put it up to him that way, then, and I'll be glad to do it.

DR. MACE: [*Repeats how he would talk to Wayne Coker.*]

DR. HYLER: I want everyone to understand I think he is going to be able to do this.

DR. MACE: We wouldn't talk to him if we didn't think he could do it.

DR. HYLER: I'll talk to him, then, and I'll alert him that you're going to talk to him too. One thing that bothers me, don't you think we emphasize this talking and promoting a little bit too much around here.

DR. MACE: Well, that is just the way things are. Now, Wayne has to overcome this southern attitude. He is a typical southerner, slow talking and a guy that needs a jolt every once in a while to get him out of his shell. He has done well as an officer of the local section of the American Institute of Chemists. It certainly seems that he has the ability, but we have to open up the flood gates.

DR. HYLER: In other places where he has been he has gained a good deal of respect because he does operate with reserve.

DR. MACE: He has to be convincing if Mr. Shaver calls him in on something. It is not a matter of making an off-the-cuff decision, he just has to express himself better. If you take a look at the other department heads, you will see what I am driving at. Knowing a subject well isn't enough, you have to be able to express yourself.

DR. HYLER: You can do just so much in trying to change Wayne.

DR. MACE: He has been six months on the job now. He certainly should feel a certain amount of security. Now is the time for him to come out of his shell.

DR. HYLER: You have no objection if I tell him we've been considering him seriously for department head, but there's this one thing.

DR. MACE: Sure. [*Laughs.*] It really did show up, Jack, at that cracker barrel meeting. You know what kind of a meeting that is, and he was the only one who didn't talk, and this was the perfect place for him to talk if he was ever going to. I recognize that this is a problem with scientific people generally.

DR. HYLER: This point of view that we have been talking about is a very prevalent one in our company.

DR. MACE: Sure, it is because that is mostly the way we operate. Other companies operate differently than we do, but we have all of these committees; therefore, it is more important here because of the wide participation which is necessary on the part of every individual.

DR. HYLER: I used to go to some length to encourage this, so much so that I am afraid that it became a detriment instead of an asset. I think that I found that trying to change the characteristics of individuals has been

working against me. In talking to my men, I found that this idea of having to do a lot of talking and selling of their programs is one of the things they didn't like.

Dr. Mace: This is the difference between working at the bench and being in a supervisor's position, particularly in this company. In this company even the men at the bench on program teams are thrown into meetings where they have to talk about their work. If they don't want to do that and can't talk about it, they'll have to stay at the bench. But when Wayne gets to the point where he is being considered for a department head job, it becomes very important.

Dr. Hyler: I remember Dr. Ross [former director of laboratories]. He was a capable man who did not find it necessary to do a great deal of talking. After other people had spent a great deal of time talking and getting nowhere, he could get to the crux of a problem in a few words.

Dr. Mace: We are not talking about making snap judgments, just the ability to express yourself on your feet.

Dr. Hyler: One of the things connected with that is, that a scientist can't say some of these things. He feels they're just not true.

Dr. Mace: I don't mean that. You don't have to lie about something. For instance, if I were at a meeting with Mr. Shaver and he asked me if we could do such and such on one of the programs—well, it seems to me that all I could say in these circumstances is that we at the present are doing the best thing that we see to do and we have every hope that it is going to accomplish the desired result. [Mace expanded on this somewhat.] If I sit there and say either "yes" or "no," or nothing, he figures I don't know anything myself. It is sort of like coming in for an interview for the first time. If a man doesn't talk, I am much less apt to think he is a good candidate.

Dr. Hyler: Gee, that scares me. [Laughs.] If Einstein came down for an interview, we probably wouldn't hire him.

Dr. Mace: No, we probably wouldn't, but it's different in industry than in academic work. There is a tendency for nonscientific personnel to judge a person on what he says and how he expresses himself. That is because they simply can't use the criteria that we scientists use. We just ask if something is sensible. I tell you that once Wayne overcomes this reserve and starts talking out, he is going to be happier around here himself.

Dr. Hyler: I just want you to recognize that he is a sound, conscientious, cautious individual who may not want to change all this much for what he will see as a temporary gain.

Dr. Mace: We're not asking him to change the soundness of his thinking. He just has to let people know that he knows what he is talking about. [Dr. Mace talked again about the Myers-White incident.]

Dr. Hyler: People who make judgments like that can do a lot of harm.

Ralph Myer's actions in that particular matter had a very bad effect on Peter White. Peter really carried the ball on that program and nobody around here knows it.

Dr. Mace: Sure, but he didn't give anyone the impression that he carried the ball. It is difficult to defend these men; they don't defend themselves. People will go around saying, all right, Guy Mace says that such and such is so, but I wonder if it really is so, just because he says it. We have to develop men around here to the point that we don't have to defend them all the time. I don't want to have to defend my men.

Dr. Hyler: I don't think we can avoid that.

Dr. Mace: Look at the other department heads. We don't have to defend them. They talk up for themselves.

Dr. Hyler: Basically what you said then is—[Dr. Hyler repeated what he was to say to Coker pretty much as he had before.]

Dr. Mace: Yeh, that will be fine, and I hope you'll let me know when you have talked with him, and I'll talk to him some more about it.

Several days after this conversation, while talking to Dick Payton, the researcher from the Harvard Business School, Dr. Hyler expressed his feelings about the situation. He said that he believed that quiet men generally made the best research chemists and that it was a mistake to want to hire nothing but extroverts. He believed they should have some appreciation for the differences between people—that if they hired nothing but the kind of men Dr. Mace seemed to want, they would be risking a stereotyped, nonproductive group.

He also felt unhappy about having to ask Wayne Coker to change his way of behaving. His experience had taught him that men did not like to have such changes forced on them, and he was particularly unhappy about asking Coker to make such a change because he believed most people respected him as he was.

In spite of his feelings to the contrary, it was clear to Dr. Hyler that Dr. Mace expected him to talk to Coker along the lines he had stated. Dr. Hyler planned to do this during the course of his regular weekly meeting with Coker, and he told Dick Payton he would be glad to have him sit in on this meeting.

When Coker came to Dr. Hyler's office several days later for the weekly meeting, therefore, Payton was present and recorded the pertinent portion of the conversation between the two men as follows:

Dr. Hyler: I have one other item. You recall our conversation about your becoming assistant department head, and during that conversation how we reviewed the probable sequence of events. Since then, I have talked to you several times about your work, and I think, have made it quite clear to you that I was very satisfied with the way things have been working out and that I intended to recommend your promotion to department head. I want to tell you that I have made this recommendation.

In discussing my recommendation, Dr. Mace and Mr. Wright have made one comment that I want to discuss with you, and I know that they will probably want to talk about it with you, too. Now, you must understand that there is no question of your competence. We are all agreed that you have been doing an excellent job. There is one factor, however, that we want you to think about. That is, in your participation in committee meetings and team meetings, we would like to see you expand a little bit. We have talked about this before. (*Laughs.*) Let's put ourselves in their shoes. A committee meeting around here is organized to help the chairman. Sometimes merely bringing up speculative ideas triggers someone else. Dr. Mace and Ralph Myers, and people like that, like to see people participate. When you don't, they have to say, "Well, old Wayne didn't say much—wonder what's the matter. Maybe he doesn't feel good today." The next day when you don't say much, though, they might think something different. It is part of the responsibility of a supervisor and something you will develop. We have talked about this, as I have said, and I am sure we are agreed that many times you can say just a little and be very helpful. There are other instances, however, where general talk will, as I say, help trigger other people's minds, and you should think about that since that will be a prerequisite for additional responsibility.

COKER: That's very true. (*Laughs.*)

DR. HYLER: You and I, of course, have a pretty easy relationship. We see each other every day and we can talk things over more than one time. At committee meetings, though, they are trying to stimulate thought, and each person present has to contribute.

COKER: I agree. I just don't like the idea of talking to hear myself talk. There is too much of that already. The change from where I didn't have an opportunity to where suddenly I have it has been pretty marked. I really don't feel that I have much to offer yet; but certainly, your point is well taken.

DR. HYLER: Dr. Mace knows you are getting your feet on the ground, but now is the time to work on this. It is definitely a prerequisite to further advancement. You and I don't feel this need so much in our relationship as we bat things back and forth. We can talk our problems over repeatedly. I suspect that this is pretty much the same thing as selling.

COKER: I appreciate the need for that. Another aspect of it is the brash young man who dashes in where angels fear to tread and spouts a lot of junk.

DR. HYLER: Well, we have talked about that before. You know the necessity of doing this sort of thing, and it is the time now to be doubly conscious of it. The fact that this point has been brought out doesn't change the fact that our operation continues to go along exceedingly well. This is just another hurdle for you to fly over.

COKER: Let's hope it isn't the number of words you say that counts.

DR. HYLER: I am sure that it isn't. It's just a desire on the part of everyone for you to get in there and participate on an equal basis with the rest of the people. There's no question in my mind that you can do this. Think it over because I think Dr. Mace will want to talk to you about it. It's almost like getting into a Sunday school play. You're kind of bashful till you get in, but once you get on stage, you are all right. Well, Wayne, that is all I had for today.

Several days after this conversation, Dick Payton had a chance to talk with Wayne Coker about it. Coker was very anxious to talk with someone about the situation in which he found himself, and as a result, the two men discussed his problem for almost two hours. Payton believed that Coker was both disturbed and frustrated as a result of his conversation with Dr. Hyler. He talked about the history of research at Industrial Chemicals and expressed his opinion that the morale of the research organization had deteriorated in the years he had been with the company. He believed that this had happened because of increasing pressures from top management, which had resulted in overorganization of the research program and a consequent stifling of freedom to work on interesting leads.

In regard to his own situation, Coker made the following statements during the course of the conversation:

I can't change my whole way of operating overnight the way they are asking me to. Furthermore, any change I can make cannot be in the direction of destroying my integrity as a scientist. I really don't approve of a lot of talk unless a real contribution can be made. I sometimes think there is too much talking going on around here anyway.

This is a company that is oriented toward product development, and I can't make much of a contribution in that area. In these meetings I go to, I have to wait until the talk gets around to organic chemistry before what I have to say is worthwhile.

I have some pretty strong feelings about what is wrong with the research effort around here, but I don't believe I should talk about these things in meetings when Dr. Hyler is there too. If something should be said, it is his place to say it.

When I'm talking about scientific facts, I feel like I can say exactly what I mean and know I'm right. But I'm somewhat fearful of talking in other areas, where a smooth talker can outtalk me and make me look foolish.

I realize there is a difference between saying things that are not scientifically accurate and putting what you say in terms that nontechnical people in management can understand. There is certainly a real need to sell research in this organization, and I am for it 100 percent. It's just a question of methodology—how you go about doing that.

I recognize that I am a man poised on the brink of a cold lake trying to make up his mind to jump in. I guess it is me that is at fault here, not the organization. People are available around here, and you don't get your head chopped off for making mistakes. I've just got to decide how far I want to go and how fast. I wish I could get some help, but I'm not sure where I can turn to get it.

Chapter 13 (Continued)

*INDUSTRIAL CHEMICALS, INC. (B)**

BY THE TIME he had been employed for eight months as head of the insecticides department, Dr. Corey Washburn, in the spring of 1954, was giving serious thought to his own future and to the potential of Industrial Chemicals in his special field of research. It appeared to him that the attitude toward research that seemed to exist in the research and development division of the company made it unlikely that his department would ever be able to do the type of work which he believed might lead to important discoveries. He worried also about his technical development and his reputation as a scientist, which he believed were factors that were not taken seriously enough by management. Finally, he felt that management did not fully understand the potential of his field nor did they appear to consider seriously his recommendations concerning the program he believed vital as a minimum research effort in this field if any results were to be forthcoming.

Industrial Chemicals had, for many years, been successful in marketing a number of chemical specialties which were largely the product of brilliant development work. The research effort, as opposed to development, was relatively new to the company, and the insecticides department was the newest field to be represented in the research laboratories. The department had been initiated three years before Dr. Washburn was hired as its head. Most of the early work had been based on developmental projects rather than on fundamental research. The original department head, Dr. Holden, had been promoted to a higher position in management early in 1953, after having built the department up to a considerable size. Several months before Holden's promotion was announced, research management decided that it might be wise to initiate a modest program of basic research in the insecticides field and started looking for a competent scientist to lead the department. During the course of the next ten months, they interviewed a dozen men and finally decided to hire Corey Washburn.

Washburn had just been promoted to associate professor at a well-known university and had behind him a distinguished academic career for a man of 35. Since he had returned from wartime service in the Signal

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Corps, from which he had been discharged with the rank of major, he had earned his graduate degrees and had published more than 30 technical papers, an output which the director of research laboratories, Dr. Hyler, considered unusual.

Washburn had become impatient with his situation at the university, however, largely because he believed his immediate superior to be incompetent. Faced with a number of years of further service under this man before he was due to retire, Washburn decided, although with some misgivings, to accept the position offered him at Industrial Chemicals.

Some eight months later, Dick Payton, a researcher from the Harvard Business School who was studying the administrative problems of Industrial Chemicals' research and development division, interviewed Washburn in his office. Payton had previously interviewed Washburn several months before. He had been interested in the way the scientist had expressed himself in the first interview, and wondered if his ideas had changed in the intervening period. The following is a transcript of the second interview from Payton's notes:

PAYTON: I don't really have any questions. What I want to do is to get you to talk some more like you did the last time I spent some time with you. I am particularly interested in what you have to say because you have been here only six or eight months, and it seems to me that you have had some problems working into the organization. I wonder if you would like to talk about that a little bit.

WASHBURN: When I came into the organization there was really no effort to indoctrinate me. I felt as if I had been thrown right into the middle of the pond and told, in effect, to start swimming. Any time I did contact anyone, of course, I got the answers I needed. No one, however, said to themselves that here is a new man who we have to help get acquainted. Then, apparently, Dr. Hyler decided that I should get to know people outside of the laboratory a little bit, and one of his assistants came around and started to introduce me to people, which I guess was a good thing as I look back on it, but at the time I thought there was something wrong with me that they were trying to correct. No one seems to realize when a new man comes in, he needs help. The initiative was left pretty much to me.

PAYTON: Do you find that this is still true?

WASHBURN: You know, Dr. Mace [*director of research and development*] got the notion that everyone should go to all committee meetings as a sort of an introductory process. So I have been auditing a number of committees, and I am about halfway through that program, and I found it very valuable and important. As you know, everybody around here says his piece at committee meetings. As a result, you can get pretty familiar with what is going on around the organization by listening in on this process. I could say that they should have done this first, but on the other

hand, there are still areas in my mind that aren't completely clear so that quite often I don't get the full benefit of what I am hearing. On the other hand, I can get all the poop that I really want. There is really no excuse to feel isolated any more.

PAYTON: So you feel that you are beginning to work into the organization and get pretty familiar with the way things work?

WASHBURN: Yes, but in the process I have committed a few boo-boos. For example, when I first came, they told me that I was going to get another PH.D. in here. In fact, at the suggestion of Dr. Hyler, I was looking for the man I wanted to hire before I even reported for duty. When I actually got here, I definitely wanted to hire a man or at least keep looking for one, but they said, "Let's wait a few months," and in general, gave me to believe that they were taking away something they promised me. In my state of paranoia I directed a memo to Mr. Wright [*business manager of the R & D division*], whom I hardly knew. As a result of this boo-boo I got to know Wright all right, but shortly thereafter, Dr. Hyler called me in and I perceived that there had been a sudden rash of meetings to talk about this memorandum of mine. In the end, they let me go ahead and get the PH.D. so you might say that I cashed in on my naïveté. I definitely got the feeling from them, however, that I was being told there were channels around here that I should learn to follow. You see, I knew that I had done this wrong, but I didn't know how else to do it.

PAYTON: During our last conversation, you appeared to be concerned about the amount of research being done in your group. How does this matter appear to stand now?

WASHBURN: Personally, I feel caught in a squeeze as far as this section is concerned. This is mostly due to the financial situation and to a lack of research success in insecticides. After reviewing the situation in insecticides here, I have spoken to several people in no uncertain terms and written some memos. Generally, I am trying to get more people into this work and to expand the effort in the insecticidal field. But I find that there is not much that can be done. They can't help it, and it is awfully frustrating. The blank check that I thought I was going to get just hasn't been realized. Of course, this is partly their ignorance. Anyone in the field should know that one PH.D. doesn't mean that insecticidal chemistry will surge forth in this company. There is not much I can do. On the other hand, I am faced with ignorance as to the real needs, and on the other hand, with the restrictions of funds. I have decided not to make an issue until more funds appear to become available. Research-wise Industrial Chemicals has traditionally done nothing in insecticidal chemistry. You know, it really takes a lot of guts to sink large sums of money into something. Other companies who have been really successful in this field, have done so, however. It's a simple fact that it takes money and facilities to win this game. Another thing that bothers me a lot is the fact that if I as an individual am not allowed to do real scientific work, I can't help losing respect for myself as a scientist. You see, you want to be doing

something that you can talk about outside the company. That you can write papers about, and so forth. Some guys are willing to go along with the kind of work they are doing here, but I just can't. I've got to know that in due time I can get up on my hind legs and talk about what I have been doing and have something that is worth talking about. Now Dr. Hyler [*director of research laboratories*] knows this. He knows that no good man is going to be happy very long under these circumstances. He'll just get stale. Another thing—management here feels that you get adequate stimulation from consultants and seminars. That just isn't so. The real guts of your stimulation comes from the colleagues around you, and in this restricted environment, this is almost impossible. Larger organizations know this, and they support research work that, although it will not bring them any short-term gain, will keep the scientist at the top of his field simply because he is able to do this kind of work.

PAYTON: You are worried that, if you cannot work on research that is more basic or fundamental than that you have been doing, you will lose your skill and capacity as a scientist?

WASHBURN: For instance, you take when I go to a meeting. I just feel ashamed if I can't talk about some real work. People will stop thinking of me as a scientist. They'll think of me as just somebody's marionette. If you don't keep up in your field, by doing some basic work, meetings just don't help. You get out of the mainstream and you are lost. It's hard to find even among scientists guys who feel this way. Too many PH.D.'s, the young ones, these days are willing to slip down into applied research, or worse. They sit here and collect their pay, and gradually the fine edge comes off their minds. Young men coming into this sort of an environment will never learn anything new. Of course, the size of the company dictates this to a certain extent. If we had some strong representation at the top-management level who could express this for us, we would be moving more in that direction.

PAYTON: One of the things wrong here is that because no top technical man expresses these views, your attitude does not stick at the top-management level.

WASHBURN: That's right. Dr. Mace certainly is concerned about the scientists under his supervision, for at a recent meeting he brought up for serious consideration the factors involved in maintaining high morale among research scientists in industry. He expresses a genuine interest in the problem, but I don't think he really quite understands it. After intensive training to solve scientific problems, you come to industry and find you are a minor cog in the wheel. Management, in effect, says, "If you're so smart, why don't you get the answers." You have got to have a top man who will say, "That isn't the way research works. This is the way it has to be done," and make it stick. Another thing—they ask too much of us, considering the people they let you hire and the facilities they give us.

PAYTON: You feel pretty strongly about these things.

WASHBURN: I feel strongly about everything. I don't react to anything mildly. Maybe I'm warped. I don't go home and listen to a hi-fi set or go out in the garden or anything like that. I don't have any hobbies. I'm not condemning hobbies. I just work 16 hours a day instead of 8 hours a day like most of the people around here. Even at the university I was told to take it easy. "Go see a movie once in a while," they said. Well, I figure that life's just too short. I can do that sort of thing when I retire. I feel that as long as you are working on something, you have to do it right. And don't misunderstand me. I am not brilliant. It's just plain hard work for me. I'm just not smart enough to get the right answer easily. I don't make these speeches just to hear myself talk. I really mean it. I told Dr. Mace when I came in I wanted the opportunity to say what I thought. He said, "Hell, the door is always open, come on in and tell me what you think." Well, I can live with this, even though it is difficult to do. You know, around here they have a personal appraisal every six months. I expected Dr. Mace to call me in after six months and tell me where I stood. Sure, he gave me a bonus at Christmas time, but I'd rather have a talk with him than a bonus. Since no initiative has come from him, it makes it pretty hard for me to go in and talk to him about these things. It isn't often the lieutenant just goes in to talk to the general. They keep worrying about people's feelings. As a result, the overall morale around here is good, but in critical areas where people have feelings, you can't get heard. You have to get knocked down, dust yourself off, and get up and try again. I don't want to seem like I am picking on Dr. Mace. We are all dying to help him around here, but he won't let us. That's what we are here for. Life is too short to wait around and sit on your fanny all day long. You were overseas, weren't you? (PAYTON: Yes.) Well, do you know anything about the third army under General Patton? (PAYTON: Yes, something about it.) You probably heard their motto. "The difficult we did yesterday, today we are working on the impossible." You can talk yourself out of doing an experiment anytime, but hell, the only way to prove anything is to do it. You have got to get your observations instead of talking about those you might get. Patton had a cocky outfit, and I admit I have been cocky everywhere else I have been and I am going to keep right on being that way here. If I can't, I'd blow my head off. It's doing things that is important, not talking about them all the time.

PAYTON: You feel sort of like a little boy who has been spanked and told that he has been naughty.

WASHBURN: In a sense, yes. I try to tell them, I point out what other people in the field are doing. But then I come right up against what Dr. Hyler says, "Sure, we know that you can do a hell of a good job with a quarter of a million dollars, but we just ain't got a quarter of a million dollars." Well, hell, that's his problem. They hired me to tell them what to do, not how to get the money to do it. Dr. Hyler has listened patiently to me on this on a number of occasions and he told me twice that insecticidal

chemistry is an area of fringe interest at Industrial Chemicals. Well, that's not much of a professional objective for me.

PAYTON: You would like to get in there and really start pitching and you're pretty frustrated because you can't do it.

WASHBURN: You're darn right. You know, I have a modest research program. Where do you suppose most of the money for research goes? Well, I end up spending less than a quarter of it for personnel. The rest goes for overhead and God knows what else. The whole trouble is if I argue on management's grounds, they can talk me into a corner. It's only if two people are talking on the same ground that they can get anywhere when they are discussing things. We just don't believe in the same things. We're not trying to get the same job done, and then another trouble is with the little money they do give me they demand impossible things. How can we get an answer out of nowhere? Give me the dough and I'll pit my gang against anybody. I can't pit a flyweight against a heavyweight though. There is just no tradition here in insecticidal chemistry. Now mind you, it's not hopeless, it just takes time. Most people think patience is a virtue. I don't. I think it is just an excuse.

Shortly after this interview with Corey Washburn, Dick Payton had several opportunities to check management's feelings about the department head of insecticides. The first opportunity came when Payton mentioned Washburn to Dr. Hyler, the director of research laboratories and Washburn's immediate superior. Dr. Hyler contributed to the total picture a few facts about Washburn's background and some further information about his experiences at Industrial Chemicals.

He said that Washburn had been brought up on the South Side of Chicago and had made his own way through college and graduate school with a brilliant record. When Dr. Hyler had suggested to Dr. Mace that Washburn be hired to fill the need for a department head, Mace had interviewed Washburn and had agreed with Hyler's opinion of him. He had been worried about Washburn on two counts, however. First, he thought that Washburn might want to do more in the basic research area than in applied research or development, and second, he was afraid that his natural drive and enthusiasm would make him pretty hard to control. Dr. Hyler had promised Dr. Mace that he would be able to keep Washburn's enthusiasm within reasonable bounds, and on this condition Mace had told Hyler to go ahead and hire the chemist.

Dr. Hyler told Payton that it was unfortunate that research policy had changed very shortly after Washburn came to work, with the result that a program of basic research on which Washburn had been counting was dropped. This had hit Washburn pretty hard, and Dr. Hyler felt that he had not been entirely unjustified in believing that the company had gone back on a promise. Unfortunately, also, Washburn had made several mistakes in this first few months with the laboratories. He had directed

memorandas concerning ideas for research in his department outside of channels, which had upset a number of people in the management group. Furthermore, he had expressed some of his ideas in rather vehement terms to several people who resented his attitude and who Dr. Hyler felt would not soon forgive him.

On the other side of the picture, however, Dr. Hyler pointed out that Washburn had done a superb job of organizing his department. Whereas Dr. Holden had needed an administrative assistant to handle details for him, Dr. Washburn so arranged the work of the section that this extra man was no longer necessary. Furthermore, it was apparent that he had gained the liking and respect of his subordinates. "In fact," said Dr. Hyler to Payton, "his juniors came to him and asked him to give lectures to them on insecticidal chemistry on their own time. So far as I know, that's the first time that has ever happened in the R & D division."

Dr. Hyler believed that in so far as Washburn was concerned, his own responsibility was clearly that of holding his subordinate's enthusiasm down to a reasonable level while still keeping management aware of the high potential of the insecticides department under Washburn's leadership.

Several days after his conversation with Dr. Hyler, Payton was able to observe an incident which shed further light on Washburn's case. Payton was sitting in the office of Dr. Mace, director of research and development, when Dr. Holden, former head of insecticides and now an associate director of the development laboratories, came in to request that a research program similar to that which had been dropped shortly after Washburn came to work be authorized for the insecticides department.

Mace pointed out that the budget for the year was already fixed and that there was little chance of an increase next year. He added that he didn't think Washburn realized, with his academic background, how fast money gets spent in industrial research. In a university, he pointed out, Washburn had gotten used to having his work done by graduate students, with little or no expenditure for salaries. He said he thought Washburn was a little confused—that he needed more experience with the kind of work that goes on at Industrial Chemicals and that if he was given time, he would develop a well-balanced research program that could be carried on within his budget limitations.

Dr. Holden questioned Dr. Mace concerning the various aspects of insecticidal research the company might be interested in investigating and then pointed out that the real need was to pep up Washburn—that he was feeling discouraged at his lack of progress and needed a lift. Dr. Mace said he felt that what Washburn needed was to face the facts of life—that actually he had been given a good deal of help. He had requested several expensive pieces of equipment recently and they had been given him. Mace went on to say that he thought Washburn had misunderstood the financial picture—that he had been made to feel that the budget was very

rigidly enforced and that this was a mistake on his part. He concluded the conversation by saying, "I just want to make this point clear. Everybody wants something new—everybody wants a new man—everybody wants money for new work. What can I do—I simply cannot exceed this year's budget until sales pick up."

When Dr. Holden had left the room, Dr. Mace pointed out to Payton that, as the former head of insecticides, Holden was inclined to have a strong sympathy with Washburn's views and that the real point of his coming in had been to try to get Mace to do something about what Holden thought was Washburn's low morale. "Maybe I should," said Dr. Mace. "This is something I will have to give some thought to."

Chapter 13 (Continued)

*INDUSTRIAL CHEMICALS, INC. (C)**

Dr. Graham Holden[1] was associate director of development laboratories in the research and development division of Industrial Chemicals. Early in 1954, he received from top management the approval he had been awaiting on Special Project X. Dr. Holden had been active in promoting the idea for this project for more than a year and was now anxious to get started on the organization of a project team.

Special Project X was to be in effect a task force group which would work primarily on evaluating new product ideas. The project team's function would be to look for new ideas both within the company and outside, and to screen out those which appeared to hold particular promise for commercial development. Having done only enough preliminary investigation to establish this potential, the team would then turn the resultant chemical compound over to the regular development staff for processing through to production.

Holden believed that it was essential to have an organic chemist as a member of the project team. In the past, new product ideas had been processed through the organic chemistry department in competition with work on regular research programs. This had not worked out satisfactorily from Holden's viewpoint. The formal programs usually received priority over new ideas and the delay in processing Holden's work was often a considerable one. Holden had talked several times with Dr. Hyler, director of research laboratories, about this situation, and the two men finally agreed that, should management approve Special Project X, Holden would request that an organic chemist be assigned to the project team on a full-time basis.

When this approval was received, therefore, Holden told Hyler he was going to request that a chemist be assigned to the project. Dr. Hyler asked if he had any particular man in mind for the job and Holden said that he hadn't really thought about it—that his only interest was in getting a good man on a full-time basis.

Holden's request was in the form of a memo to Dr. Mace, the laboratory director. Dr. Mace knew that Special Project X had received top management's approval, and he had a good deal of respect for Graham

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[1] All names have been disguised.

Holden. In discussing the matter with the case writer, he said, "Graham has a lot of ideas and the personality to sell them. He is one of the most energetic men I know."

When he received Holden's memo, Dr. Mace called Holden on the phone in order to clarify in his own mind the background of the request. He asked Holden if he had discussed the matter with Dr. Hyler and was told that Hyler knew of the request and was in agreement so long as he got a replacement for whatever man he was to lose.

Dr. Mace reviewed in his mind the various seniors in the organic chemistry group and decided that Howard Tarlow possessed the sort of background and experience that would be needed in the work with Holden. Tarlow had been with the company for eight years and for the greater part of that time had been doing chemical development work.

Following his conversation with Holden, Dr. Mace asked Dr. Hyler to come to his office to talk about the proposed change. Dr. Mace did not look forward to this interview with any particular pleasure because in the past he had found that Dr. Hyler was inclined to disagree with him on a good many of his decisions. Dr. Hyler had been the leading organic chemist in the company for 20 years, in the course of which he had won the respect of many scientists for the excellence of his work. Through the years, particularly before the tremendous expansion of the past decade, he had participated to a large extent in the planning and coordination of the company's research program. Dr. Hyler, in conversations with Dick Payton, had indicated that he felt it to be his responsibility to speak frankly when he doubted the wisdom of a proposed course of action within the laboratory. He also felt that in too many instances decisions affecting his section had been made by his superiors without his being consulted in advance.

Dr. Hyler told Payton that in the past he had been criticized by the senior chemists in the research laboratories because his interest in the work of the laboratory made it difficult for him to delegate responsibility to his scientific group. From talking to some of the seniors in the organic chemistry department, however, Payton gained the impression that Dr. Hyler seemed more recently to have earned the respect of his men by his willingness to listen to their complaints and his attempts to change his own behavior in order to meet certain criticisms.

When Dr. Hyler came in, Dr. Mace told him he wanted to assign Tarlow to Special Project X. As Dr. Mace had expected, Dr. Hyler brought up several objections. First, he did not believe that Tarlow, being quiet and reserved, had the type of personality to be successful in working with Holden. Dr. Mace replied that, as he saw it, the two men would complement each other perfectly. He believed that the new job would tend to bring Tarlow out of his shell and, on the other hand, his methodical approach to research problems would balance Holden's ambition and drive. Dr. Hyler said he did not believe Tarlow would change

very much; that he was surprised to hear of Tarlow's interest in this sort of job and would like to talk to him about it. He asked Dr. Mace what would happen if Tarlow decided to turn this job down. Dr. Mace replied that he personally would consider this a sign that Tarlow did not wish to progress in the organization, and he would probably never be offered such a chance again. The interview terminated with the decision that Dr. Hyler would discuss the proposed change with Tarlow and report the outcome to Dr. Mace.

In talking with Dick Payton about the events that followed this conversation, Dr. Hyler said:

"I was far from convinced that Tarlow was the right man for this job, but the decision had apparently been made and I had to follow through. I'm pretty proud of the fact that I was able to present the proposed assignment to Tarlow objectively without revealing my own bias. I felt he had to make the decision himself. He wanted more information and decided to talk to Graham Holden. After he talked to Graham, he came to me and said he had decided to take the job. Well, I was pretty surprised—I had really thought his interests lay in other directions, and I told him so. For one thing, I didn't want him to hear through the grapevine that I had opposed him for the job. I told him I had thought he was more interested in research, but that if this was what he really wanted, I would consider the matter closed and would tell Dr. Mace he wanted to take the job, which I did. Well, Tarlow slept on it overnight and then came in the next morning and told me he had changed his mind—that he wanted to stay where he was."

Dick Payton was in Dr. Mace's office when Dr. Hyler came in to tell Dr. Mace of Tarlow's change of heart. He recorded the following conversation:

HYLER: I wanted to talk to you because Howard Tarlow came up to see me today and told me he had changed his mind about the Project X job. He decided it would take him away from chemistry and the work he really liked to do in the laboratory. I had talked to him yesterday and explained what the job would be like, and when he went home last night, he seemed to want to do it. Now, this morning he has changed his mind, which seems to me to mean he would rather stay in chemistry.

MACE: I want to talk to him and explain that this would mean passing up a very valuable opportunity.

HYLER: He asked if it made any difference to his future, and I said as far as I was concerned, it didn't.

MACE: He should know that this would mean he would have to be satisfied with a salary ceiling from now on, that he could never get out of the laboratory.

HYLER: I'll be frank. I didn't tell him he was prejudicing himself in the eyes of management for the future.

MACE: I just wanted to be sure he knows what management's reaction

will be when he turns this job down. If he is happy in his own mind about doing this, why that's okay with me.

HYLER: I told him I was damned glad he made the decision now instead of six months later. I told him also that I wanted him to know that my view all along has been that this work would not be what he would like to do. He went home overnight and came back and said he worried about what the job would be like. He was afraid to get away from work he was best fitted for in the laboratory. I sympathize with him. After all, there is no reason he couldn't be a section leader some day.

MACE: Well, I am not too sure about that. There would be a question if he turns this down. The chance is that he would never advance much further than being a senior scientist.

HYLER: I can't look at it that way. I think the guy was wise.

MACE: You have got to remember some of the other instances like this in this company. Now you take Graham Holden. When he was the company's radio-chemist he was offered the administrative job in development. He took it and he has gone on upstairs. But you remember the man in analytical a year or so ago who was offered an administrative job and he turned it down? Now, he isn't going to get another chance. I just want to point out to Tarlow that these opportunities occur from time to time and that when a refusal is made, that pretty well closes the door for future opportunities.

HYLER: I really give him credit for coming back and saying he had changed his mind. I think it takes a lot of courage to do that.

MACE: Well, it is better for a person to know where he stands before too many years, so I want to be sure that he knows exactly what he is doing. After all, the whole thing may be academic. We had a meeting the other day and we were told that the budget had to stand at 2.3 million for 1954, which is a substantial reduction from what we had asked for, and this is probably going to mean that we will have to cut out some things. It may be impossible to make this shift anyway. I won't know definitely until next Wednesday because it is a matter of company policy.

HYLER: Well, there is no need to get into a discussion of what we do now then. Do we want to bring this opening to the attention of others who might really want to do it? I know one individual who would obviously fill the bill. Of course, I don't know if he would take it, but it seems to me that Otis Clare would be excellent for this.

MACE: I guess that depends on whether this would be okay with Graham or not. We'll have to see.

HYLER: In the meanwhile, I will continue to keep Howard busy on development work.

MACE: Okay. I'll call him in sometime soon and talk to him.

Dr. Mace was disturbed by this conversation. He wondered how Dr. Hyler had presented Special Project X to Tarlow, and he felt responsible for assuring himself that Tarlow was aware of the serious consequences

which turning the job down would inevitably have on his future with the company. The next day, therefore, he asked Tarlow to come to his office. The conversation between the two men ran as follows:

MACE: Howard, I have been talking to Dr. Hyler about this Special Project X job, and I wanted to talk to you to find out how you were feeling about it. I have thought all along that you would be the one who would be most suitable to work on this Project X as a result of your experience in development work and your excellent chemical background. It was on that basis that we felt it desirable to select you. I want to give you some information on the background of this particular project. [*He described Holden's work on setting up Special Project X and detailed the extent of the proposed operation.*] Now, I understand that you have talked to Graham and to Dr. Hyler, and up till yesterday, I had the impression that you were interested in this project. But then yesterday I heard that you changed your mind. I was disappointed to hear this, and I wondered why. I had seen this as a great opportunity which would bring the person doing it to the attention of top management.

TARLOW: Well, I know that there is room for a lot of work there. I decided, however, that it was not for me because it would take me away from chemical work and give me instead a lot of routine work on synthesis. I would have to do the same sort of thing over and over on different compounds, and this wouldn't help me to develop as an organic chemist.

MACE: Well, it wouldn't be as routine as straight analytical work. Actually, in a way, it represents a challenge considerably outside of the routine. Since Graham is not a chemist, ideas would have to flow from you to him. Graham might bring a lot of new ideas from Europe on which you would have to try to develop improved methods of synthesis, and there would be all sorts of opportunities for doing interesting work.

TARLOW: I don't suppose the thinking would be routine, but the bench work would be.

MACE: Would it be more routine than on a regular development project?

TARLOW: Yes, I think it would.

MACE: Oh. I begin to see. What you are interested in doing is a series of exploratory syntheses rather than a single synthesis on each project.

TARLOW: This is really a new kind of job entirely, isn't it?

MACE: Yes. [*He talked about the difference between the work that would be done by the organic chemist assigned to Special Project X and normal development work.*]

TARLOW: Well, it seems to me that my work would result in maybe two or possibly a dozen set reactions that would have to be tried on any compound. Furthermore, it seems to me to do this work well, I would need to know something about polymers, and this is a little out of my field.

MACE: Would you be interested in learning about that?

TARLOW: Sure, I would.

MACE: I wouldn't worry about it then. The people you will be working with don't have very much knowledge about polymers either. As they work with it, they become more familiar with it, and gradually they will get to the point where they are pretty conversant. I wouldn't make it a major worry that you don't have this particular background. I wouldn't be reluctant to accept this position just because of that.

TARLOW: The question in my mind is, how do I want to broaden myself. I decided last night that I wanted to gain more experience in organic chemical research.

MACE: Well, in light of that, would you prefer to work on research programs rather than on the processing work you are now doing?

TARLOW: Yes, as a matter of fact, I have wanted to do that for a long time.

MACE: Well, I'm glad I talked to you then. These conversations sometimes bring out what people really want to do. If your interest is purely in developing new methods of synthesis, you might be better off upstairs in the research department.

TARLOW: Yes, I have always had that desire. Of course, this does not mean that I am not happy where I am.

MACE: I didn't mean to imply criticism. Dr. Hyler has always said you have been doing excellent work on your present job. I was just exploring where you might be happier. Often when you enter an organization in one type of work and then work for a year or so, you find a greater interest somewhere else in the company. If this is so, it is often better to put people where they will be of more benefit to the company and themselves because of their greater interest in a particular kind of work.

TARLOW: Coming back to this other work with Graham. It would appear to me that the wheels of motion on this would take quite a while to start turning.

MACE: Well, I have already requested a replacement for you in chemical processing. Then as you train a new man, you would gradually go over to Project X 100 percent.

TARLOW: That's not my point. The preparatory work that a person would have to do, it seems to me, is the important thing. On each new idea he would have to search the literature and spend a lot of time getting oriented before he could do the bench work. The working out of new reactions in that field would not be difficult, but results would not come any faster than it would upstairs in research. Wouldn't the development people get pretty impatient waiting a year for results?

MACE: No, they wouldn't at all. This is a challenge and it would take a year or more to work into the job properly. The thing is that when one real product comes out of Project X, that would prove to management that this is the kind of operation that can succeed. Then you would get more people working with you. And you've got to realize also that your

work would be recognized faster in this Project X assignment than if you went to research.

TARLOW: You think this position has more advantages than a job in research, then?

MACE: Yes, it is a new thing, a challenge, and it puts you in closer contact with management than either regular development or certainly research work would. Therefore, you will get greater recognition more quickly. Although I must recognize your work in your present job, others in management just don't understand research, and they wouldn't be able to appreciate what you were doing.

TARLOW: Maybe I had better reconsider here. (*Sighs.*)

MACE: I don't want to influence you. I just want to clarify the situation. Let me tell you that this is not a fly-by-night project. Graham is full of ideas, which is good. He needs to be tempered by the experimental approach, however. That is where you come in. Of course, it is still up in the air so far as implementation goes. [*He explained the budget situation to Tarlow.*] On the other hand, if you would like to reconsider, I wish you would do it. If we don't go into this immediately, it will come up again, and we would want to consider you whenever it does.

TARLOW: This is the type of thing that it is hard for me to make up my mind on. It isn't too clear-cut yet.

MACE: No, these things seldom are in their early stages. We have a lot of ideas, but we are not at all definite as yet. We just have to play along with it and see how it goes.

TARLOW: Well, I'll think it over some more.

MACE: This has been a good conversation. I've gotten a good idea of what you like to do, and if this doesn't work out, we will want to look upstairs and see if there is anything for you up there. This Project X, though, is one chance in a lifetime, and it isn't likely to come along again.

TARLOW: This Project X isn't likely to fold up in a year or two, is it?

MACE: No. Graham will be pushing this, and management will certainly never hold it against you if it doesn't work out.

TARLOW: Well, Graham and I would get along fine together. I'll think it over again. Things get kind of rushed in my section, and all this has hit me rather suddenly.

MACE: Yes, well, that is why I wanted to talk to you. In talking to different people, you get different interpretations. I wanted to be sure you were straight on this.

In talking about this conversation with Dr. Tarlow to Dick Payton, Dr. Mace expressed a number of thoughts. He said that it had seemed to him that Dr. Tarlow needed (*a*) to be sold on the challenge and the potential of the Project X job, (*b*) to be reassured that he was able to handle this kind of job, and (*c*) to be reassured that he would not be blamed by

management if they changed their minds about Project X—in other words, that this would be a secure job for him. Dr. Mace then talked generally about how men come from college with ideas derived from their college professors. They feel that they want to do pure research, often along specific lines. He did not think that this was very realistic in so far as industry was concerned, since it was very seldom true that a company would have a need for the particular kind of work that a man had been doing in the academic atmosphere. The academic atmosphere was perfectly all right, but it was pointed toward training men in research in a specific field rather than toward producing something of value to a company.

Dr. Mace felt that it was his job as director of the research and development divison to show men under his supervision other roads they could travel that could also be fascinating and challenging although based on different values than those they had heard from their professors. He felt that it was a part of his job to generate a certain amount of excitement and stimulation in men in development work as against pure research work. Quite often, he believed, by the time a man had been working for the company for some years, and had been exposed to a good deal of this challenge and excitement, he would shift his values and find the same excitement in development work that he had found in following through a complicated research project in the academic atmosphere.

Dr. Mace pointed out that he himself, when in college, had taken a course which was called "The Theory of Analytical Chemistry." He felt that the title was misleading and that it had been in reality a course in which the professor told them about the differences between chemical research as done at the university and that normally done in industrial laboratories. Dr. Mace believed that this course had been very helpful to him personally in making the adjustment from the academic to the industrial atmosphere.

Chapter 13 (Continued)

*INDUSTRIAL CHEMICALS, INC. (D)**

DURING THE MONTHS he spent studying the activities of the research and development division of Industrial Chemicals, Inc.,[1] Dick Payton, the researcher from the Harvard Business School, recorded a number of observations that centered around the relationship between the director of research and development, Dr. Mace, and the director of development laboratories, Dr. Myers.

Dr. Mace, in the spring of 1954, had occupied his present position for about four years. He was the third man to hold the job in ten years. In the early '40's, the director had been Dr. Ross, a man who had earned from his subordinates a tremendous amount of respect and genuine affection. His personality was frequently recalled by scientists in the division who invariably characterized him, when talking to Payton, as someone who had had a real understanding of the needs of scientists while at the same time being able to maintain the confidence of top management.

When Dr. Ross died suddenly in 1948, his place had been taken by Dr. Tuckerman, who accepted the job after resigning from a similar position in another company. In the two years that Dr. Tuckerman directed the work of the division the overall morale of divisional personnel slumped badly. A number of people accounted for this, while discussing the situation with Payton, by referring to Dr. Tuckerman's insistence that all activities of the division be highly organized and rigidly controlled.

Dr. Mace had replaced Dr. Tuckerman in 1950. He, too, had been retained from outside the company, and he also had come from a similar position in another company. Dr. Mace, however, had spent a good many years as a professor of chemistry at a well-known university before taking up scientific administration in industry.

Dr. Myers, on the other hand, had come with Industrial Chemicals 20 years before as a young graduate chemist. He was the first scientist assigned to work on development problems, and for several years he worked alone in a small laboratory which had been set up for him in an old warehouse several miles from the main plant and offices of the company. In these early years he came into frequent contact with the top-management group, who depended on him for the technical imple-

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[1] All names and certain other factual data have been disguised to conceal identities.

mentation of their product development ideas. Gradually, as both the company and the R & D organization expanded, he was authorized additional personnel and by the time the company moved into a new modern building in the early '40's, he found himself the head of a sizable development organization.

Dr. Myers was very much aware of the fact that he had had a good deal of experience in development work, and in the spring of 1954, was wrestling with the problem of how to retain the responsibilities he had undertaken. The day that Dick Payton interviewed him for the first time, Dr. Myers had just completed work on a set of job descriptions which included not only his own job but those of his subordinates. He expressed his feelings on the matter to Payton in the following conversation:

MYERS: You have got to realize that I have been here almost since the beginning—for more than 20 years, and Dr. Mace only came in about four years ago. In these 20 years I have gotten used to a certain way of operating. I have always done things pretty informally and have been in the habit of following up a lot of things myself with manufacturing and sales people because it has been my experience that this was the easiest way to get things done. It has recently become apparent, however, that we are going to have to formalize what I do a little bit more than has been so in the past. Don't misunderstand me—there is lots of room for initiative in the company. In fact, it seems like people go out of their way to avoid crushing anyone's initiative. I have been used to doing things my way and no one ever tells me not to do it, so I just go along and operate the way I always have. I don't kid myself, I know that I have 20 years of background, and in 20 years I have learned a lot about how to handle things in the development field here at ICI. The trouble is, I go to Dr. Mace occasionally to check something having to do with manufacturing for instance, because after all, as director, he is supposed to handle relationships between R & D and sales people and R & D and manufacturing people, and what happens usually is that he says to call a meeting next week to talk about the question I have brought to him. Well, now I don't see any reason for meeting and delaying things that way when I could handle the whole problem myself with the manufacturing people very easily the way I have for years.

PAYTON: You are getting signals that Dr. Mace wants to handle some of these things?

MYERS: Yes, and the trouble is that in many cases these are things which could be handled more directly by myself or other people. For instance, he told me he wanted me to coordinate the work on several polymer projects. But then, people from the manufacturing and sales division will call him, quite naturally, of course, and he will go ahead and handle things in connection with polymers directly with them, and then forget to tell me anything about what he has done. Well, what he is doing

is—he is telling me I should handle polymer projects, but then he is going ahead and doing a lot of it himself. This makes it pretty hard for me to figure out just where I stand. Even though I realize he is the director and supposed to handle relations with other divisions, I have been doing this sort of thing for years, and I feel competent to take the responsibility of continuing to do it. I also realize that sometimes it is difficult to delegate responsibility. I find myself doing things that my subordinates should handle. I just believe we have got to get these things understood and made formal. That is why we are getting up these job descriptions.

PAYTON: You have always operated pretty informally, but now you see the necessity for getting your job on paper.

MYERS: I guess everyone does a lot that no one knows about. I am sure Rossian does a lot that I don't realize he handles, and I am sure my secretary does more than I realize until she goes away for a two-week vacation. Certainly, I doubt that Dr. Mace understands the responsibilities I have undertaken over all these years.

PAYTON: You are not sure your boss appreciates all you do around here. Does this worry you?

MYERS: No. Not particularly, except it makes for inefficiency and duplication. I have always had the technical background to handle my job, but now I have this problem of how to deal with Dr. Mace and I don't know quite how to handle it according to best personnel practice.

The preparation of the job descriptions referred to by Dr. Myers in this conversation had been initiated by him in an attempt to clarify his own responsibilities in relation to those of Dr. Mace. In forwarding these job descriptions to Dr. Mace, Dr. Myers had written a covering memorandum which included this paragraph:

> You will note that some of the duties listed among those of the director of development are presently officially part of those of the director of the R & D division. However, since they are all essentially developmental, I had been doing them before you came. I have continued to do a large amount of this work, since it seemed logical, efficient, and expedient. I am confident that I am capable of assuming full responsibility for these operations and reporting to you on those that need your attention the same as I do any other development matter. This should not only relieve you of some of your load, but should also eliminate duplicate effort. I believe that it would facilitate the overall operation if they were carried out by a person one step nearer the bench. I have been concerned for some time due to the lack of clear-cut responsibilities and the authority to do that which I really felt I should be doing. I am sure that a clarification of responsibilities and definition of authority would remedy this.

Payton had several opportunities to sound out Dr. Mace concerning the relationship between the two men. On one occasion, several weeks after Dr. Myers had forwarded the job descriptions to his superior, Dr. Mace had this to say in answer to a question of Payton's:

MACE: Ralph Myers is a very ambitious man who is unhappy with the realities of his present existence. I am certain that he was bitterly disap-

pointed when he did not get this job after Dr. Tuckerman left. Unfortunately, although he does a good job on development work, and has for many years, he just has not got the capacity to go any higher. He not only rubs a lot of people the wrong way—which is the main reason I have been taking over the relationships with sales and manufacturing—but he doesn't seem to be able to learn. For instance, there is a particular type of report he is supposed to write up and submit to me periodically which is circulated all through the division and to other divisions as well. On a number of occasions I have called him in and gone over these reports with him and pointed out specific changes I wanted him to make in his method of writing up the report. Invariably, the next report comes through with the same errors in it. Another thing—he still operates as though development was a one- or two-man operation. He can't seem to keep out of the laboratory. He is constantly getting into details of the work that should be handled by his subordinates. Furthermore, he is constantly trying to assume responsibilities that rightfully belong to me as director of the R & D division. One clear indication of this was the job description he wrote up recently. Dr. Hyler wrote up one that was quite good, but when Ralph's came down, it was instantly apparent that he had not written the description of his job as it is, but as he wanted it to be. In fact, when my secretary brought it in to me, she said that it looked as though Dr. Myers wanted to take over my job.

Payton was particularly interested in this last point because he recalled that during the course of a conversation he had had with Dr. Myers a week or so before, Myers had said specifically that he did not feel badly about being passed over on the promotion to director of research and development. He said that he realized that this position required a broad background which he knew he did not have. On the other hand, he was strongly opposed to the gradual relinquishing of responsibilities which he believed Dr. Mace was demanding of him.

Payton made a point of keeping in touch with developments concerning the relationship between Drs. Mace and Myers in the two months he remained at Industrial Chemicals. He recalled particularly a conversation he had with Dr. Mace the day he concluded his observations in the laboratories. Dr. Mace, was, at that time, even more worried about what to do about Dr. Myers than he had been two months before. He said that Dr. Myers had been getting worse in his relations with manufacturing people—"throwing his weight around" and arguing points vehemently in meetings to the point where Dr. Mace believed either name-calling or physical violence would result soon if something were not done.

Dr. Mace was wondering particularly how he should handle two specific interviews with Dr. Myers that he knew he was going to have to initiate soon. First, he had not yet discussed with Myers the job descriptions Myers had submitted to him. Second, a supervisory appraisal system

had recently been initiated in the R & D division, and in the ordinary course of events the postappraisal interview with Dr. Myers should be scheduled within the next month or two. The possibility of dispensing with Dr. Myers' services was not in the picture at all, according to Dr. Mace. Not only had he been of great value to the company for 20 years, but he was, from a technical standpoint, an excellent man in the developmental field. Dr. Mace believed that somehow he had to get Dr. Myers to accept the new order of things, but he had no idea how he was going to go about accomplishing this task.

Chapter 13 (Concluded)

INDUSTRIAL CHEMICALS, INC. (E)*

In common with the rest of the research and development division, the development laboratories had "grown like Topsy" as Industrial Chemicals, Inc.,[1] expanded. The organization as it existed in the spring of 1954 represented a very considerable gain in both personnel and facilities, particularly if compared with the situation as it had been 20 years before when Dr. Myers was working alone in a small laboratory which had been set up for him in an old warehouse.

The major portion of the development work was handled by a group of senior scientists who led teams of juniors and technicians assigned to various projects. On technical matters, these seniors reported formally to Dr. Myers through Dr. Rossian, who had been appointed associate director of development laboratories less than a year before. On certain other matters, however, particularly those concerned with project coordination, budgets, and relationships with other divisions of the company, they were supposed to work directly with Mr. Wright, the business manager of the research and development division, or Dr. Mace, the division director.

The two oldest seniors in terms of service with Industrial Chemicals were Marvin Dorrance and Preston McCann. Dr. Myers had hired Dorrance nearly 15 years before as his first assistant. Dorrance had only a Master's degree, but in those days nobody worried very much about a man's academic background. As Dorrance expressed it to Dick Payton, the researcher from the Harvard Business School, "We were overloaded with work and I just got into more and more projects as I went along. I picked up most everything I know by a process of osmosis. Ralph Myers helped me a lot in the early days, of course."

McCann, on the other hand, had been hired just before the war to work on a particular project. He had a Bachelor's degree in chemistry and had worked for a number of years in another research laboratory in the same industry. Within two years after coming with Industrial Chemicals, he had come up with what was then believed to be a successful solution to the problem he had been assigned when he was hired. The result had been a high-volume product which soon led management to request development

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of a group of subsidiary products using the same basic process. When it had been applied to the subsidiary products, however, it soon became apparent that the process he had developed was inherently weak, although it had been the major factor in the success of the original product. In 1954, therefore, McCann was trying to work out an alternative solution to the problem, and he had working with him the largest group of juniors and technicians in the development laboratories.

Dick Payton spent three weeks interviewing and observing the activities of various people in the development laboratories. In addition to the material he obtained from Dr. Myers (Industrial Chemicals [D]), he was particularly inerested in the statements made by Dorrance, McCann, and a third senior in the development laboratories, George Jankowski, who was the most recent addition to the group. Selected portions of Payton's notes on his interviews with these three men follow.

Marvin Dorrance

Marvin started by talking about his work and telling me what he does. It seems that he is the senior man in the laboratory. He told me that when he first came with ICI, they were in the old laboratory and the company was a lot smaller than it is now. He pointed out one major difference between the way he operated then and the way he is doing now. In the old days he was responsible not only for the chemical work on products but also for the analytical work and even work on budgets and other administrative details. As the company has expanded, Marvin pointed out, they have, however, developed a section that does nothing but analytical work, and the administrative details have been taken over by administrative people. He feels that this is all for the good since it allows the development chemist to concentrate on his main function instead of having to be bothered with details that can be performed adequately by service groups.

He asked me if I knew how new products came into being, and when I confessed that I was somewhat fuzzy about this, he outlined the process for me. He pointed out that the idea for a product often starts with their sales force, who tell the laboratory people about some need in a particular field which should be met by a new product or, in other cases, men at the bench come up with suggestions for fields of work they believe might be of potential merit. A number of such ideas become elective work for the scientists in the laboratory. In other words, before such ideas are formalized into projects, a scientist may elect to do some preliminary work on it simply because he happens to be interested in that particular field. As a result of this, by the time a project goes into the exploratory stage, there is often someone in the laboratory who has done a good deal of preliminary work on it. Following the exploratory stage, if as a result of exploratory development the product looks as though it may have considerable

potential, it goes into regular development. From that point it follows the usual course through pilot and production to the market.

Marvin pointed out that as a result of his long experience in the development laboratories, and the various elective projects that he had worked on over the course of a number of years, that he had more or less grown into a lot of special jobs which no one else in the development group was likely to be doing. He meant not only that no one else did these specific things but that no one else worked on so many varied little special projects. For instance, one of his special fields was technical promotion. Over the years he has been working with a representative from the sales department and a representative from the plant as a committee to consider the promotion of all ICI products. As he talked about promotion, Marvin showed a good deal of enthusiasm for this subject. He pointed out samples of various advertisements and promotional devices which were around the walls of his office and told me something of the factors which had to be considered before such pieces were approved.

Another peripheral project which Marvin has been engaged in for some time has been an attempt to find out if their present stability tests are adequate. Apparently, this came up as the result of one of their products a year or so ago having to be called in from the market because for one reason or another the stability testing had not shown up a fault in this product which did show up after it got on the market. Involved in this work on stability testing is an attempt to work out a series of tests which, while they do not take an inordinate length of time, are adequate to prove the stability of a product. This work requires that Marvin find out, among other details, the extremes of weather conditions all over the world. He had several books on his desk which he showed me that he had been studying to find out what these extremes were so as to try and reach some sort of judgment as to what effect these figures had on their stability testing program. Summing up, Marvin said, "I guess you can tell that I really enjoy my work here."

Talking again about his background in development work, Marvin pointed out that in general he knew more than Rossian, more even than Ralph Myers who has been there in the laboratory longer than he has, in regard to much of the work he was doing. He said that it was his responsibility to let Rossian and Ralph know anything that he discovered which might have serious repercussions, but that he had a good deal of leeway in regard to his work because experience had proven that his conclusions were apt to be good ones. He pointed out that because of his background in the old days, when he had to do analytical and budgeting work on projects, he automatically thought of things like costs and sales and promotion aspects of a project while other men in the laboratory would seldom consider these aspects of a situation. He pointed out that Rossian had to depend on the seniors in the laboratory much more than Ralph. Marvin said that he makes a lot of technical decisions that Rossian

never knows he makes. He pointed out that he was not running Rossian down when he said this; that Rossian just didn't have the background and wasn't as close to the various projects as he was. He said, "Of course, sometimes Rossian is apt to ask a question which is helpful and which points out something I have not considered, but 99 percent of the time I have already considered everything he brings up."

Marvin then pointed out that there were several kinds of seniors in the laboratories. He said that, in effect, he and McCann were group leaders and that their work consisted mainly of directing the work of the juniors and technicians assigned to them. He differentiated himself and McCann from seniors like Jankowski who, he said, were new men who did a good deal of work at the bench. They have no one working for them because they do not have the knowledge to be able to direct the work of others. On the contrary, they have to do a good deal of work themselves so that they can get familiar with the kinds of projects they will be directing in the future. In effect, he said that the status of the younger seniors is quite different from that of older seniors or group leaders. "The younger men are learning, whereas we are supervisors."

We then got talking about the status of the various people in the laboratories, in regard to the various degrees they had in contrast with their positions. Marvin pointed out that he himself had a Master's degree, that McCann had only a Bachelor's degree, and that Jankowski had a Ph.D. He said that, although past experience in ICI had given him to understand that you didn't have to have a Ph.D. to get ahead, things were changing. He found that men who were made department heads or project leaders these days were apt to have their Ph.D.'s, although this had not necessarily been so in the past. He said he was certain that, when and if the present department head left the analytical section, a Ph.D. would be appointed in his place, although in analytical chemistry a Ph.D. was pretty hard to find. From this we got talking about Marvin's feelings when Rossian, who is a Ph.D., was made assistant to Dr. Myers and then associate director of development.

Marvin said that he would have fired anyone who talked to Ralph Myers like he did when Rossian was made Myers' assistant. "Ralph is aces with me, but he really pulled a wrong one on me when he handled this the way he did." Apparently, Marvin had not been told about this promotion until it was announced. He stormed into Myers' office and told Myers that he wanted to know what was going on. He said that Myers was embarrassed and talked around the bush, that it wasn't until he talked to Dr. Mace that he really got the story. He says that Dr. Mace lets you know where you stand and talks frankly to you in almost every case. Apparently, he was told by Mace that he was more valuable where he was, close to the work in the laboratory. He later discovered that he was being paid more than Rossian, and this fact helped to make him feel a little better about it.

Marvin continued to talk about this situation, and his feelings as expressed to me might be summarized as follows:

"I felt that I should have been made section head instead of Rossian, but I was more deeply resentful of the promotion being made behind my back than I was of the fact that I hadn't been promoted. They told me I was valuable in the laboratory, in a sense like Dr. Kurt, but I don't see myself as a Kurt. Actually, I like the work I am doing, and if it were really true like they say that I could progress to the top by doing this kind of work, I would like to do it that way, but my experience here tells me that actually Rossian will someday be making more money than I am, and in a sense, he should, because I guess he is the one who has the responsibility. Nobody has ever told me that, but I just know that's the way it will be. I don't particularly like administrative work myself, but I do believe I could do good work in Rossian's job if that is what it takes to really get somewhere in this company. Even today, although he is formally my boss, I do not really accept Rossian as my superior in technical matters. In fact, I know more about my work than he does. I told Ralph Myers that I would find it hard to take technical orders from someone who knows less than I do. Rossian today is really more of a coordinator than our boss. In spite of this, we get along all right—you can't blame Rossian for getting ahead. I just like to know what the score is.

"Around here they say that the scientist has no ceiling, and people tell me all the time that I am the Kurt of the development labs, but actually, I don't see myself coming up with something sensational like he did, and that's what it really takes to get high up on the ladder and to get really high pay that compares with the administrative people above the department head level. In actual fact, it just isn't so that I will ever be able to enjoy the kind of future that is ahead for Rossian. Things being the way they are, I am more or less resigned to my fate, especially since, on the whole, the company treats me very well, but I must say that I envy men who can actually get in the laboratory and run experiments and who have little paper work to do but are called department heads. McCann and I, who are working in development, spend most of our time at our desks writing reports rather than working at the bench, and I regard this as pretty much of a nuisance, but I realize that it is necessary in the kind of work I am doing."

Marvin then brought up another subject. He drew a little diagram showing the project leaders reporting directly to Mr. Wright. He said that this was the way the system was supposed to work, but in practice it seldom happened that way. He brought out the fact that Ralph Myers finds it hard to get out of the laboratory. He recalled the fact that for years he and Myers have worked closely together. Myers originally started the development laboratories, and Marvin was hired in the early years when there were only a few people in the laboratories. Later on, when Myers headed up both the development laboratories and the pilot

plant, he continued to operate directly with the men at the bench. Since the expansion has taken place, there is now an associate director between Myers and the men in the laboratories, but Myers is still constantly coming into the laboratory. One result of this is that the project leader seldom reports to Wright, but instead goes through formal line channels. Marvin says he rarely goes to Ralph Myers himself these days. He goes to Rossian unless Ralph calls him in. He said that Ralph finds it hard to turn his men loose. For instance, he feels he has to make comments on all reports, even though what he has to say is not always very important. Even though he might suggest no change in a report, he will often call up Marvin on the phone to talk about a report which has reached his desk to make sure he understands what Marvin meant when he wrote what he did in the report.

Preston McCann

McCann started off by saying that since he did not have a Ph.D. and as it was impossible to get one in his field working nights, he was, of course, stymied in the research and development division and that he and all the other men with Bachelor's or Master's degrees realized this. He said not to get him wrong, that he had been treated very well at ICI and that in so far as his salary was concerned, he had absolutely no complaint, but that it had been quite apparent that they were bringing in Ph.D.'s to take over jobs like department heads, and that men with lesser degrees had very little chance of stepping into an administrative job unless they went down to the plant, where Ph.D.'s were very much frowned upon and where men with lesser degrees could get ahead. McCann went on to say that he himself was not particularly anxious to take over a lot of paper work, unless such a job carried with it a very substantial raise in pay, in which case he would certainly consider it seriously. He also said that he knew very well that Ph.D.'s were necessary—that men with this training were vital to an organization because of their special training and contacts. This is a natural process of growth in the industry, he believes, and one of the harsh realities of life older men without Ph.D.'s just have to accept. He then went on to talk about the circumstances surrounding Rossian's introduction into the organization.

His first statement about this was, "In regard to an inexperienced Ph.D. being put over us, of course he doesn't know what I'm doing and he can't be of any help to me and it would take me entirely too long to explain to him what I am doing. On the other hand, I realize that Dr. Myers has to have an assistant to handle his paper work."

McCann then went on to say that Rossian's introduction into the organization had been a very unfortunate one and that the way it was done had created a great deal of animosity in a few people, which had gradually died down but which would never completely disappear.

Apparently, as McCann saw it, what had happened was that Myers called everybody together one day and simply announced that Rossian was going to be his assistant, and this was the end of it so far as Rossian's introduction to the organization was concerned.

A summary of McCann's feelings as he then expressed them to me would go something like this:

"I have a good deal of respect for my own ability and I believe that I could probably have taken over the job of assistant and done well with it. Of course, all of us think that we can do the jobs above us and maybe we are right, and maybe we are wrong, but at least I would like to have been asked if I was interested in this particular job before getting a dash of cold water in the face, which is in effect what happened. Ralph Myers is a heck of a nice guy, but this particular situation was not handled properly. What he really wanted was someone to take a lot of administrative details off his shoulders so that he could get back into the laboratory the way he had always been and get closer to the work without having to be constantly snowed under with requests for vacations, interviewing people for jobs, and other things like that. Ralph likes to be in the laboratory all the time, following up projects he is interested in. He is a very enthusiastic guy, and I don't know whether this is good or bad, but frankly, over the years his enthusiasm has paid off. Sometimes you get pretty impatient with him because he changes his mind pretty often and work that he asked you to do last week may be dumped out in the ash can tomorrow if he gets another idea. When you come right down to it, Ralph is still behaving like a group leader. He is having trouble learning to delegate responsibility. Rossian is in kind of a peculiar position because of the animosity created by the way he came in here and because of the fact that Ralph follows up on a lot of things without telling him about them. It's hard to go to Rossian to tell him what is going on because we don't know exactly what it is that he wants to know or what he wants to follow up on. The result is that he has to come to us and ask us for the information he wants; we don't go to him.

"I don't like to be critical like this unless I can suggest some other way that may be helpful. Rossian could have gotten rid of this animosity if he had behaved differently than he did. If I had been him, I would have called all of us together when he first came in here and made a little speech. I would have told all of the men that I didn't know very much and that I was in a difficult situation and needed all the help I could get from all of the men who knew what was going on. I would have said that I recognized the fact that probably some of the men in the laboratories were hoping to get the job, and I realized that I might have stepped on some toes but that was the situation as it was and that I was going to try and work with it the best I could and hoped that I would get the support and help from everyone that I needed if I was to be effective. But, Rossian didn't do this."

(I asked him if he thought that Rossian had a pretty good idea of what the situation was when he came in and McCann said, "No, like all PH.D.'s, he probably didn't.")

I said that it seemed to me that McCann was telling me that management was evincing a certain lack of respect for men like himself and Marvin Dorrance by behaving as they did in putting inexperienced PH.D.'s over them. McCann responded with a very definite affirmative to this restatement of his feelings. He said that this whole question of PH.D.'s coming into the company had made it pretty difficult for the men in the laboratories because they were always wondering when a new man came in what it meant. He was quite obviously referring in this instance to George Jankowski, and we soon got talking about Jankowski. He said that PH.D.'s coming into the company seem to adopt the attitude that they know it all and to have the feeling that they are a group apart. He wondered why Jankowski had not done the same thing that he had done when he first came to work for ICI. McCann pointed out that when he first came in, he asked thousands of questions. Even when he knew how to write a report or at least had a pretty good idea how he would like to write it, he took pains to ask someone else how to go about this, saying in effect, "I don't know much around here and I wish you'd help me." McCann said that because he had done this, he soon found that everyone was very willing to help him, and he believed that this was the only way for a new man to break into the organization. He said:

"After all, Marvin and I know an awful lot of people around here, and we get a lot of things done just by knowing who can do things and how you have to approach certain people to get things done. Someone like Jankowski is completely dependent on us for help, even in little things like how to get these beakers sterilized, but instead of going around and asking questions and asking for our help, he does things like taking a lot of data that we had accumulated on a particular project and drawing up a 12-page memorandum, restating the data in a different way and distributing it all around the organization. Of course the reaction to this was a feeling on everybody's part that this was not very valuable; that it was just stating the data in a different way. Also he makes comments in meetings which are based on his supposedly better education, but when you come right down to it, these statements reveal a complete lack of knowledge about what's going on here at ICI."

George Jankowski

I spent the morning interviewing George Jankowski. It will be recalled that George was hired about six months ago. He had previously taught chemistry for seven years, five after he received his doctorate. He graduated from the same university as Rossian six months before Rossian did and has known both Rossian and Ralph Myers for a number of years. George said that it was a difficult decision for him to make to come into

industry, but in a way, it was according to plan. The fact that he knew both Rossian and Ralph well and liked them had a good deal to do with his coming to ICI. He had planned to stay in industry for two years and then to go into business for two years in order to get a variety of experience. He is now not so sure he is only going to stay at ICI for two years.

One reason that his decision to leave the university was a difficult one lay in the fact that he had formed a very good relationship with the group at the university and hated to give this up to come into a completely strange atmosphere. He now says that he feels that the group here is similarly easy to get along with, and he no longer feels the lack of companionship that he was afraid he might. When he agreed to come to ICI it was understood between him and Rossian and Ralph that it would be for a two-year period, at the end of which time either he or the company would decide whether or not he was going to stay on. He said that he chose ICI as his entry into industry because he felt that they were pioneering in the type of work which happened to be his primary interest. He felt that he could express his knowledge and his experience in research best at ICI and thus make the most use of his academic experience.

Since he has been with the company he has been working primarily on one particular project. He says that he is extremely happy that the project has recently been judged to be quite successful. He feels that the main reason that he has had a good measure of success with this project is that Rossian and Ralph have permitted him a considerable degree of freedom in developing an approach to the problems. He feels that the man who should know how to go about work of this sort is a man with experience in the field who is equipped with both talent and knowledge. He felt that he had sound fundamental training, but he came into industry expecting to be told what to do. At the university he was his own boss and worked on projects of his own choosing and used methodology of his own devising. He did not expect to be allowed to do this in industry, and although in some senses these expectations were realized, he is extremely grateful to Rossian and Ralph for giving him as much leeway as they have.

George feels that a scientist in industry should be equipped with the type of training and knowledge that will allow him to make definite decisions about his work. He feels that the fact that he was so well trained in scientific methods and the further fact that he was intimately acquainted with the technology involved in the project, although he had had no industrial experience in this field, enabled him to make definite statements during the course of this project concerning what should and should not be done and make them stick simply because he was confident of his analysis of the data and expressed his confidence in no uncertain terms. He pointed out, for instance, that as he has been working on this particular project, he has been using a methodology quite different from that being used in some other parts of the laboratory.

In describing the way he worked, he pointed out very strongly that one

of the reasons that he operated as he did was the fact that he was very conscious of the value of publications. He stated that work of merit, which is published, serves as a prestige item for the investigator and the organization for which he works. From an idealistic standpoint, it could be the principal basis of descriptions and claims made for new products for which public acceptance is sought. He also said that from a practical standpoint, sound scientifically published work of an applied nature could in itself be powerful advertising which might contribute to the financial rewards of the firm. Therefore, he had been extremely careful to accumulate data with a view to publication. He said that this was not the normal way of operating here in the laboratories, that as far as his standards were concerned, many projects were carried through in such a way as to leave big holes from a scientific standpoint. He believed there was a tendency to push things through at the expense of intermediate steps which were necessary for complete scientific authenticity and said that he was trying to encourage the idea that every step should be covered properly in the process of investigation. He feels that most data collected in the laboratory is incomplete; that if there is not enough data to publish, then the data frequently is not sufficiently conclusive to make good business judgments. He summarized his sentiments with the statement that work done with a view to possible publication would be both good science and good business.

In connection with his use of the best scientific method, he pointed out that the decision-making process at ICI seemed to him to take an unaccountably long time, and he felt that one reason for this was that the collection of data often was carelessly done so that there frequently was room for considerable argument and variance of interpretation of the data, which would not be so if the scientific work had been properly done in the first place. In other words, decisions took far longer than they should for the simple reason that the decision-maker did not have available the data he should have to make the decision obvious. As George said this, it seemed to me that he was talking as though all decisions were based completely on scientific evidence, and I held this one up to him. He agreed that, of course, marketing and financial considerations always came into the decision-making process. However, when making decisions like whether or not a project objective was likely of attainment in the laboratory, or whether to proceed along certain lines as against alternative lines—at these times the proper collection of data was a prime necessity in the decision-making process.

George said that he didn't want to belittle people around here, but according to his observations, much of the work was pretty empirical. I asked him how this happened to be, and he said that if I would look around in the development labs, I would find very few people with strong fundamental scientific training. He said that, in the past, a good many men with lesser fundamental background have been doing scientific work in

industry, and since they lack the proper scientific background, much of their work is done by intuition and the feel of a situation rather than systematic, scientific investigation. George expressed some amazement at how well these people have acquired the ability to make intuitive decisions which are often quite good ones. On the other hand, it seemed quite clear to him that an approach yielding quantitative scientific evidence is far preferable to this other method of operating. He said that he wasn't sure that industry really recognized the fact that they needed people strong in fundamental science to do research of an applied nature. For one thing, it seemed to him that one reason sales and management people have been telling scientific people what to do for so long in industry is that scientists have not been competent enough to be confident of their own judgments. He said that he believed that you had to open your mouth and say so when a management idea for a project was impossible or very doubtful from a scientific standpoint. He felt that having the confidence to do this was far preferable to working for three months on an unsound project. He said that Ralph and Rossian felt pretty much the same way about this, and he believed that this was one reason that he had been brought into the organization.

I asked him about his relationship with other people in the company, and he immediately mentioned Dorrance and McCann. He said that he believed these men were doing much good work, but that due to their insecure scientific position, they probably were unable or unwilling to present evidence and judgments which might tend to modify a management decision. Also, he said, "I see McCann and Marvin making mistakes. It's not for me to say that they are making these mistakes, but if I were in their positions, I know that I would do things quite differently." He said that these men, like most of the people in the laboratories, were inclined to wait until someone else used a new material or used a compound in a certain way before they did. Their background was simply not strong enough to give them confidence in developing new or unusual compounds or methods.

I went back to George's statement about not feeling that he could point out to McCann and Marvin and others that they were making mistakes, and asked him why he hesitated to do this. He said he felt he should walk lightly on suggestions of this kind until he had been here longer and established some record of accomplishment. He did not feel he could tell a man who has had 15 years' experience in a particular field that what he is doing is scientifically wrong, awkward, or cumbersome.

At this point I asked George about his thoughts for the future, that is, where he would like to go and what he would like to do careerwise? He said at first that he would like to go up the scientific ladder as opposed to the administrative ladder, but after some hemming and hawing, admitted to an ambition to be a department head, at which level he felt he could have some voice in what projects were to be worked on and which were

not. I asked him if he saw the department head as primarily a scientist, and he said that he realized that someone like Rossian does an awful lot of paper work and feels that he would not like to do as much administrative work as Rossian does were he a department head. He feels that having a department head with a strong fundamental scientific background do this type of work is a terrible waste of scientific training. He believes that any man who is responsible for making scientific decisions should be close to the work in the laboratory sense—that it is very difficult for someone who does not follow the work closely to make intelligent decisions as to the future of a project. During the course of this discussion he stated his feeling quite clearly that he felt far more comfortable making decisions about things based on scientific data than making decisions about people based on judgments he was not nearly as sure about.

He felt that a good scientist is apt to stick close to a laboratory; that the men who try to make their mark in administration are frequently those who see their own limitations as scientists. "On the other hand," he said, "I would certainly like to have my work recognized. I want to be known as a good scientist, if I am one, and I want other people besides the scientific community to know this." He said that he didn't know whether or not this was ego, but he felt that the titles held by people like Ralph Myers and Dr. Mace were important and that he would very much like someday to be a vice president of research.

Chapter 14

HAIG CHEMICAL COMPANY*

THE HAIG CHEMICAL COMPANY[1] had been manufacturing fine chemicals since 1909 in a plant at Baltimore, Maryland. The company purchased raw materials and manufactured them into bulk chemicals for use by pharmaceutical and drug concerns, food and beverage manufacturers, industries, and the arts, and by physicians, dentists, and veterinarians. The principal function of the company was a factory processing of chemicals. The plant originally consisted of a single small building, but by 1929 had grown into a large modern plant. The total net sales in 1929 were $8,100,000. Although sales dropped during the depression to $6,210,000 in 1932, by 1938 the figure had risen again to $9,700,000. Exhibit 1 gives the sales record of the company. New product developments and the war quadrupled sales between 1938 and 1945.

Before 1933 all manufacturing had been organized under an operations department, which was responsible for experimentation and research, process and product development, production methods, and factory operation. All the technical personnel were administered by this department.

Organization of Research Department

The Haig Chemical Company in the early thirties had adopted a policy of expanding its research activities as a means of insuring its long-run growth and the maintenance of its position in the chemical industry. One of the first steps in this program was the organization of a separate research department reporting to the president of the company.

In 1933 the research department had been organized to provide modern facilities for research in organic chemistry, biochemistry, physical and inorganic chemistry, microbiology, process development, and chemical engineering. In the years between 1933 and the beginning of the war, large expenditures were made for the expansion of this research program, and modern laboratories were constructed with the latest equipment and manned by high-grade scientists, chemists, and technicians. The research activities of this department were closely coordinated with government,

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[1] All names have been disguised.

medical, educational, and scientific societies and agencies throughout the country to exchange ideas and to supplement the work of these various agencies, and the research department soon began to make a substantial contribution to the pure science of chemistry.

In the years following the foundation of the Haig research department, its chemists, engineers, and technicians pioneered in the discovery, development, production, and clinical evaluation of a number of new products which represented important advances in medicine, nutrition, and industrial chemistry. Because the demand for these products, especially during the war years, far exceeded the company's ability to produce them,

Exhibit 1

HAIG CHEMICAL COMPANY

Year	*Total Net Sales*
1929	$ 8,100,000
1930	*
1931	7,150,000
1932	6,210,000
1933	7,000,000
1934	7,550,000
1935	8,370,000
1936	9,450,000
1937	10,350,000
1938	9,700,000
1939	13,230,000
1940	15,660,000
1941	25,650,000
1942	28,100,000
1943	37,530,000
1944	37,440,000
1945	37,350,000

* Not available.

facilities were expanded rapidly, and in 1945, these new products represented nearly 50 percent of the total sales of the Haig company, whereas in 1938, they had represented only about 1 percent of the sales. Largely as a result of the development and production of these new items, the sales of the Haig company grew from $9,700,000 in 1938 to $37,530,000 in 1943. Coincident with this growth in sales volume was a rapid and appreciable expansion both in plant facilities and in number of personnel. The research department had become a vital part of the Haig company, contributing appreciably to the growth of the company in sales volume and in prestige.

The research department had originally been given the overall responsibility for the development of new products and was in charge of a product from the time of its discovery until all the production difficulties had been overcome and the product had been turned over to the factory for manufacturing on a full scale.

The transition between the discovery of a new product by the research department and large-scale factory production of that item represented many steps involving long periods of experimentation, testing, processing, and designing. One executive characterized the number and variety of problems that arose during this period of transition as "absolutely incredible." In the early stages of development in the research department, the product was analyzed and tested to determine possible industrial uses, medicinal application, toxicity, and curative powers, after which period production was finally accomplished in test-tube quantities. Before the product left the research laboratories, processes for larger-scale production were formulated.

At this stage, also, the development division of the research department analyzed the product to determine the advisability of producing it on a large-scale factory basis. Considerations of cost, profit, availability of materials, need for the product by industry or medicine, suitability for plant production, and possible future developments were presented to the president and the management committee[2] by the research director with a recommended course of action.

The Haig company operated on the principle that it was wise to "make all your mistakes on a small scale and your profits on a large scale." When the decision to make a product had been reached and the research laboratories had decided on the most suitable manufacturing processes, the item was taken to the pilot plant where production was begun on a small scale. In the pilot plant, the chemical engineers, aided by the chemists who had developed the product, designed and operated a small unit to test the processes for manufacturing the item. Here the processes were revised to fit larger-scale production; mechanical engineers were called in to design machinery and equipment for future factory production; industrial engineers began to lay plans for factory scheduling, layout, packaging, shipping, and flow of materials; and construction engineers made arrangements for any necessary expansion of plant facilities. Production in the pilot plant continued until the processes were satisfactory and the factory layout was ready for full-scale production. In some cases an item had been produced in the pilot plant for as long as two years because plant expansion had not been completed; the factory was therefore not ready for full-scale production, and yet there was an urgent need by the government, the armed forces, or civilian medicine for the product, even in quantities produced by the pilot plant.

With the shift of the production of the item from the pilot plant to the factory came a further period of redesign of equipment and change in

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[2] The management committee, composed of Mr. Haig; Mr. Towne, the operations vice president; Mr. Ranyard, sales vice president; Mr. Nordly, the treasurer; Mr. Ferguson, the legal officer; and Mr. Richard Robinson, the secretary, was organized for the purpose of making company operating decisions.

process to fit the larger scale of manufacture. The chemical, mechanical, industrial, and construction engineers were all active during this period in efforts to iron out all the wrinkles in the factory process. Even after the process was satisfactory and production completely turned over to the factory staff, research continued on the product and the process to discover other possible derivatives, by-products, substitute raw materials, faster or cheaper methods, and additional applications.

The organization of the research department is shown in Exhibit 2. The department was subdivided into seven divisions: three were involved with pure scientific research; two were concerned with product and process development and chemical engineering; one was a records and information division; and one was a service group for the department.

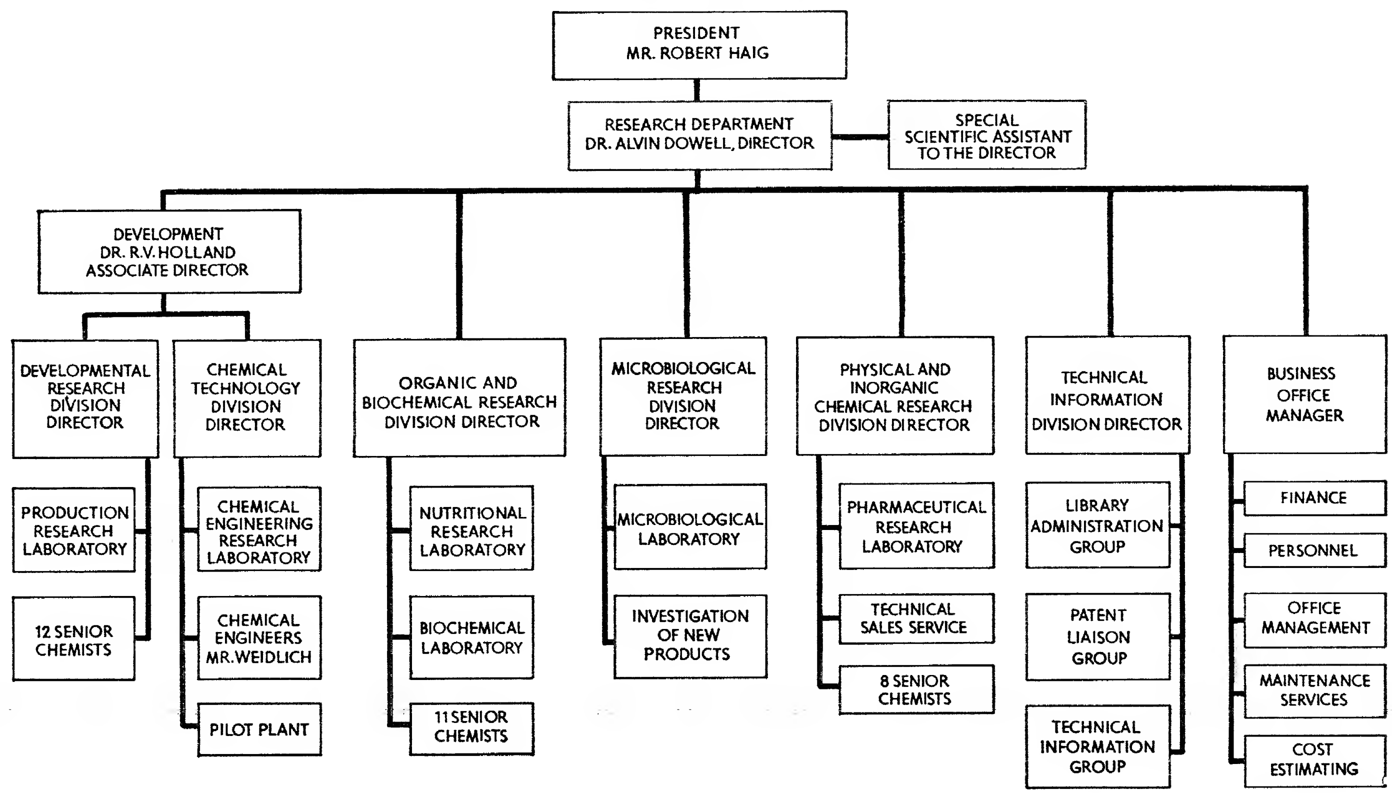
The three pure research divisions engaged in organic and biochemical, microbiological, and physical and inorganic research involving investigation and experimentation in such fields as toxicology, pharmacology, chemotherapy, therapeutics, microanalysis, entomology, synthesis, nutrition, textile chemistry, and cereal chemistry. These divisions utilized the department's thoroughly equipped laboratories for their research. Each of the three groups was under the supervision of a director who was responsible to Dr. Alvin Dowell, the research director. Senior chemists in each group were in charge of various types of experimentation or of particular product analyses. These senior chemists were all men with college degrees in chemistry or physics, many of them had doctors' degrees, and some had taught the sciences in colleges.

Several top executives of the company characterized the chemists as follows: they were singularly devoted to pure science for science's sake, and research work represented a principal objective in their lives. As a group, also, they were disdainful of the money-making applications of science to production and engineering. These chemists were individualistic in their attitudes and in their working techniques, and yet they tended to band together as a group of common interest and to set themselves apart in thinking as well as in attitude from the other groups in the company. As individuals, they devoted long hours to the laboratories and were conscientious and untiring in their investigations, becoming so absorbed in their work that they frequently became oblivious of surroundings and other people. A combination of imagination, inspiration, adequate background, and hard work was responsible for their success in research. The senior chemists were responsible to the director of their division, who was in each case a man of long experience with interests and background similar to their own. He commanded their respect principally on the basis of his scientific skill and accomplishments. The research director, Dr. Dowell, was highly thought of by the senior chemists because of his prominence in pure scientific research and the manner in which he had organized the department to provide a high degree of scientific freedom to the chemists. He worked on the theory that the success of the research department

Exhibit 2

HAIG CHEMICAL COMPANY

Research Department



could not be measured in daily output but must be considered in the light of months and years of results.

Of the two development divisions under the associate director of the research department, the development research division likewise was composed of senior chemists utilizing a laboratory for scientific process development, while the chemical technology division included the chemical engineers who were studying process application to pilot plant and factory operation. The chemists in the former division were men similar to those described in the pure research divisions except that their work involved investigation of process rather than product discovery and analysis. They were required to work rather closely with the chemical engineering group, which applied their processes to pilot plant and finally to full-scale production. There was a tie between the chemists and the chemical engineers based on the similarity of their work, but, according to nontechnical executives, the chemists as a group felt that the pure scientific aspects of product and process development were of prime importance and that the chemical engineering and development work was of a less highly skilled and scientific nature.

The chemical engineers were men trained in college either in chemistry or chemical engineering. Some had doctors' degrees, and all were fundamentally interested in applied chemistry and physics. The function of this group was to work in conjunction with the process chemists, to become familiar with the process, and to streamline it for pilot plant and factory use. Various features were considered by this group, including plant and equipment design, yield rates, rate of production, and scheduling of flow. It was evident to company executives that they respected the senior chemists in the department and were generally tolerant of the group scientific pride felt by the chemists.

In the laboratories, in addition to the senior chemists, were numerous younger chemists and technicians. Most of these were college graduates, who visioned a future in chemistry similar to that enjoyed by the senior chemists. The attitude of devotion to science was prevalent throughout the laboratory groups.

The information and business office divisions were purely service functions for the department, established to provide an ample library, patent information, technical information, and various office and control services.

The research department had been established and developed with large expenditures by the Haig company, and both the executives and the scientific men expressed their view that the facilities for both scientific work and comfortable office space were excellent. The laboratories were spacious, well lighted, well ventilated, and strictly modern; the offices and workrooms for chemists and engineers were large, comfortable, and well equipped; the service facilities were many; and the routine was conducive to uninterrupted study and investigation. The top management considered

physical conditions for research to be nearly ideal, and the chemists and chemical engineers were obviously well satisfied with the arrangements.

The Engineering Groups

In developing a product from test-tube to full-scale factory production, the research department had the staff assistance of several groups of engineers. In addition to its own chemical engineers, it had the services of mechanical engineers, industrial engineers, and maintenance and construction engineers, as well as the factory production staff when needed. These engineers and the factory staff were under the line supervision of Mr. J. E. Towne, the operations vice president of the company.

The mechanical engineers were college-trained men for the most part, with degrees in mechanical engineering. This group was primarily concerned with the design of machinery and equipment for the pilot plant and factory. Upon specifications furnished by the chemical engineers and determined by observation and experiment, the mechanical engineers designed, procured, and installed such items as pumps, cylinders, piping, pressure chambers, heating units, refrigeration units, cranes, pulleys, and the hundreds of other pieces of equipment involved in chemical processing. These engineers were required to work in close conjunction with the chemical engineers who were charged with setting up the process in both pilot plant and factory. The chemical engineers as a group considered the mechanical engineers as a less highly skilled group performing a service function.

The industrial engineers were probably the most heterogeneous of all the engineering groups, largely because industrial engineering is one of the more recently developed and accepted engineering fields. In this group were men, mostly college graduates, who had been trained in mechanical, civil, or commercial engineering and in business or economics, as well as men trained only in night school classes. The men in this category were the least specialized, the least technical and perhaps the least important engineering group in so far as new products were concerned. The functions of the industrial engineers consisted of scheduling production, planning flow of materials, providing packaging and shipping facilities, and making time and motion studies. The industrial engineers worked with the chemical engineers to determine the length and type of process, the skills involved, the safety precautions required, the packaging needed, and details for the time and motion study. The industrial engineers contacted the mechanical engineers regarding layout of machinery, speed of operations, and operators required. The industrial engineers similarly worked with maintenance and construction engineers in the design and construction of plant facilities involving the flow of materials and the speed of operation. Company executives believed that the chemical engineers looked upon the industrial engineers as a nonscientific service group

instrumental in the details of scheduling and planning. The mechanical and industrial engineers were closely related in background and general interest, and usually agreed readily on matters of machinery layout and operation.

The maintenance and construction engineers were a group of mechanical, civil, and electrical engineers, college trained for the most part, although some were noncollege men who had experience in contracting and construction work. The responsibility of this group was primarily the construction or alteration of plant facilities to accommodate new or revised processes and to maintain facilities throughout the plant. When a new process was scheduled for factory operation, the maintenance and construction group in conjunction with the other engineering groups, and in accordance with specifications of the chemical and mechanical engineers, constructed the required machinery foundations, extended the factory, revised ceiling heights, and provided service piping. This engineering group had the lowest percentage of college-trained personnel and had a less vital part in new product procedure than the other groups and was principally involved in plant maintenance, construction, and alteration in accordance with independent specifications. The chemical engineers, in particular, were not accustomed to working in close conjunction with this group.

The factory production staff was responsible for the actual operation of production processes. This group, composed of chemists and some chemical engineers, specialized in the problems of equipment operation and efficient production. They were strictly an operational group and did not assume responsibility for a product until the research department relinquished its control of the products' development and turned it over to the factory. In the initial establishment of a product in the factory, however, the factory staff assigned men to work with the chemical engineers as well as with the mechanical, industrial, and construction engineers, in instituting the process and in learning the details of the operation preparatory to assuming full control of the factory production. The viewpoint of the chemical engineers that chemical perfection in the product and process was essential sometimes proved to be at variance with the practical viewpoint taken by the factory engineers, who held that compromises must be made to adapt the process to factory convenience and scheduling.

Organization of Engineering Departments

Before 1943, there had been no concerted effort to unify all the engineering functions within the company into a centralized engineering department. Each group of engineers, except those in the research department, had reported to the operations vice president. The industrial engineers were a separate group under T. B. Watson and the mechanical,

construction, and maintenance engineers were directed by R. A. Merrill. Each group had a particular function to perform. Each group usually exhibited a fractional point of view on new product development and there was not so much intergroup understanding as might have been desirable. The management considered, however, that the organization was functioning tolerably well, in view of its rate of change and growth.

As the company grew rapidly and added new products and expanded its plant and personnel, the lack of engineering centralization became more and more noticeable. The increasing scale of operations, the problems of plant expansion, and new products increased the work of the operations vice president in coordination of factory activities. This in turn reduced the time he had for coordination of the work of the engineering groups at the very time when the volume of that work required not only more supervisory attention but possibly some further degree of specialization. The engineering groups continued to work together and with the research department on an informal basis of past practice. The results during the period of rapid expansion were as follows: many instances of duplication of effort, lack of coordination between engineering groups, inadequacies of data and statistics, and waste of time caused by conflicts of responsibility and authority between the engineering groups. Sometimes engineers were assigned to a job for which they were less well qualified than other engineers. For instance, the research department frequently asked mechanical engineers to handle certain projects that might better have been handled by industrial engineers. There was no central control functioning effectively to determine responsibility and authority, to assign jobs, to avoid duplication, to gather data centrally, to centralize engineering facilities, to settle disputes, and to act as an expediting agent in carrying through proper action to conclusion.

An example of the duplication and inconvenience coincident with the noncentralization of engineering facilities was the drafting department. The engineering drafting department was located about one quarter of a mile from most of the engineering groups in the plant. Some of the groups did their own drafting, to avoid sending prints back and forth to the drafting department; other groups were forced to make it standard practice to send a girl on a bicycle at regular intervals to the drafting department to deliver or pick up drawings and blueprints. Although the inconvenience, annoyance, and loss of time to the engineers were obvious, no action had ever been taken to correct the situation.

An excellent example of the errors and inefficiencies occasioned by the lack of centralized control of engineering was illustrated at the time of the construction of a new plant near the existing plant in Baltimore. In the construction of the plant building, the chemical engineers had required in their plans and estimates a 14-foot unobstructed overhead clearance. After the building had been constructed, however, it was discovered that service piping required for the floor above had to be placed below the ceiling and

thus the unobstructed height was reduced to 10 feet in some locations. This costly error was entirely due to the fact that no central control was placed on the engineering phases of the construction, to check such features and to insure that the chemical engineers and the construction engineers had coordinated their efforts on the design and erection of the building.

Top officials, in addition, referred to the existence of friction between the chemical engineers attached to the research department and the other engineering groups. This friction centered on the question as to which group was more qualified to handle a new product through the pilot plant and the factory. One of the chemical engineers expressed the reaction of the group when he said to one of the factory chemists,

> Of course, it's a research department job to supervise a new product till the factory methods are straightened out and all the wrinkles are gone from the process. Dr. Dowell is responsible for it up to that point, and we belong to the research department, so we'll keep the job till it's completed and then we'll turn it over to the factory. You factory people don't know anything about research, chemistry, chemical engineering, or the history of the discovery and development of this product. How can you be trusted to make it till we show you how?

The factory chemist countered by saying,

> You research people should stick to your test tubes. Chemists and scientists have no business ever leaving the lab. You certainly don't understand factory operation and that's why we are having so much trouble getting these new items produced. You keep thinking you are in a lab making 20 cc.'s instead of in the factory making thousands of pounds. We know the factory setup. All we need is the process; we'll do the rest!

In 1943, the company's management committee began discussions on the question of the consolidation of all engineering groups into a single engineering department. Mr. Towne, the operations vice president, was particularly active in promoting such a reorganization. Mr. Towne was able to convince the committee and Mr. Haig, on the basis of facts and figures which he presented, that the existing decentralized engineering organization was responsible for numerous duplications and inefficiencies. He suggested that in a $37-million business, engaged primarily in technical production, it was essential that the engineering function be centralized under one leader and unified as to purpose, methods, and controls.

At Mr. Haig's request, Mr. Towne had a study made and a report prepared by several of the principal company engineers which indicated the prevailing weaknesses of the existing engineering organization and proposed changes needed to accomplish engineering unification. This report was submitted by Mr. Towne to Mr. Haig and to the management committee. It provided that all engineering, including chemical engineering now under the research department, should be centralized in a new engineering department under the operations vice president.

When the plan for the proposed engineering department became known throughout the plant, there was outspoken opposition from the chemical engineers and a rivalry among the other engineering groups as to which was to play the most prominent part in the proposed engineering department. Mr. Weidlich, head of the chemical engineering group, made his attitude known to Mr. Merrill, the head of the mechanical and construction engineers when he said,

> We don't feel that we are nearly as much engineers as we are chemists. Our particular job is new products, and we work as closely as possible with research. By the time these products become factory problems for engineers, we are through with them. I can't understand how you figure we could be separated from the research department and still carry along these new products. It just doesn't make sense. I think the rest of the mechanical, industrial, and construction engineers might consolidate within your own department and call it an engineering section, if you want, but I'm certainly opposed to including all chemical engineers in that group because we just don't belong.

Dr. Dowell, director of research, when consulted on the issue by Mr. Haig, the president, expressed himself as opposed to the change involving the chemical engineers. Dr. Dowell explained his attitude when he said,

> I've consulted with Dr. Holland, my assistant, and we agree that it is undesirable from our viewpoint to divorce the chemical engineers from our organization as long as we are charged with new product responsibility from lab to factory and we must depend so heavily on these engineers for the accomplishment of the job. It's inconceivable to me that such a program would secure results if I were forced to call on another separately run department to accomplish the critical features of a job for which I'm held responsible.

In December, 1943, the Haig company engaged the Garfield Company, management consultants, to study the management and organization problems occasioned by the rapid growth and expansion of the company. One of the first issues facing the consulting firm was the question of the proposed new engineering department. For several months the Garfield Company representatives familiarized themselves with the existing organization, the methods and procedures of research and production, and the inadequacies and deficiencies of the system. Finally, in June, 1944, the firm recommended to management that a consolidation of all engineering functions, including chemical engineering, be effected as soon as practical. The Garfield Company also recommended that an experienced engineer be brought in from the outside to assume the duties of chief engineer.

Although the report of the Garfield Company was similar in most respects to that submitted by Mr. Towne, the management felt that the investigation of the problem by the consultants had been of value. In the first place, management had been able to confirm its own decision on the issue, based on the report by company engineers; and in the second place, the presence of the Garfield Company as well-known consultants had given the question a position of importance throughout the plant and had

done much toward convincing the research department and the chemical engineers that action was necessary. Although most of the chemical engineers still opposed the change, the recommendations made by the Garfield Company, as unbiased consultants, partially convinced them on the merit of the contemplated centralization of all the engineering functions.

Mr. Haig agreed to the recommendation that the company hire an outsider as chief engineer. He thought a new man would be able to work more cooperatively with Dr. Dowell, who had opposed the transfer of the chemical engineers. The new chief engineer required experience in chemical, mechanical, industrial, maintenance, and construction engineering or a knowledge of how to use these types of engineers effectively. The growth of the company had created a need for a new type of technical chief. The management, not anticipating such rapid growth, had not given its various engineering heads a chance to acquire this broad experience, with the result that the top management doubted the possibilities of promotion from within to the new position. Given a new chief engineer with capacity for leadership, the management believed that any disappointment the old chiefs might feel would be offset in part by the fact that the expansion would give each of them greater responsibilities, a larger volume of work, and larger organizations to supervise. A new man, it was thought, might approach the question of division of responsibility between research and engineering with less bias than an insider.

Accordingly, in January, 1945, the Haig company employed Mr. C. P. Ryan as chief engineer and charged him with the responsibility of organizing the engineering department; that is, centralizing all the company's engineering groups and functions, establishing the responsibilities of the engineering department in the company organization, and developing procedures for the satisfactory coordination of the engineering function with the other departments of the company, especially the research department. In accordance with its philosophy of management, the Haig company allowed Mr. Ryan great flexibility and freedom of action in his efforts to establish this department. Mr. Haig instructed him in general on the results desired, but was neither prepared nor did he desire to give Ryan definite responsibilities and courses of action. Mr. Haig stated the company philosophy in a few words when he said, "We want to accomplish these changes by evolution rather than revolution. We want to let them develop gradually with as little heat from friction as possible. We can't be sure now of the ultimate goal, and to freeze a procedure or policy before it is seasoned would be bad business. We must crawl before we can walk."

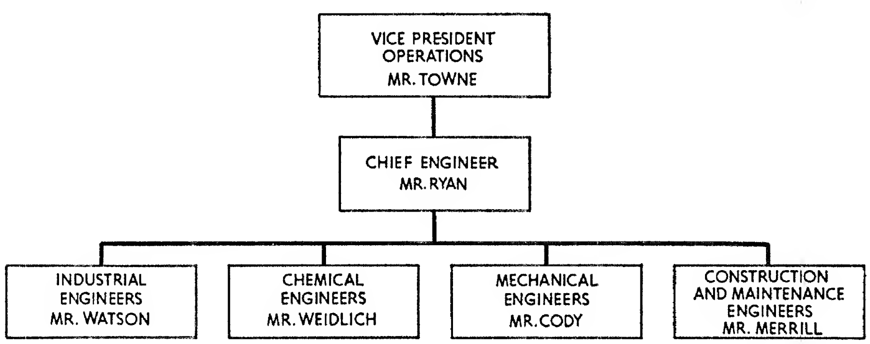
Mr. Ryan was a man about 50 years of age who had been vice president of a small chemical manufacturing company in charge of engineering and manufacture. He had had nearly 25 years' experience in chemistry and chemical engineering and was experienced in the techniques of chemical production. He was later described by Mr. Watson as "a hard-boiled sort

of man who knows his job and who's not an easy man to talk down. He's reasonable and logical, but will never abandon a thing that he believes in. He's exactly the man we've needed for this job, especially since there's been so much opposition to the plan."

Because he was convinced that one of the important steps in centralizing all engineers into a compact organization was to accomplish a physical consolidation of all engineering functions, Mr. Ryan laid plans for moving all Haig engineers, including the chemical engineers, into the same building. In June, 1945, the engineering department moved into the new building, which included desk and office space, drafting and blueprint facilities, an engineering records room for the collection of data and statistics, and a conference room.

There were minor problems of adjustments to the new space setup. Mr. Watson, who as head of the industrial engineers was in charge of planning and laying out the new engineering department office, had planned to avoid the use of cubicles for individual offices. Although the use of glassed-in cubicles was prevalent in the other company office buildings, Mr. Watson thought them a waste of limited space, and, in addition, he believed that more work could be accomplished in an engineering office without the obstacles to informal communication presented by glassed-in offices. Previously, the chemical engineers had enjoyed unusually good office space in the research buildings, and the loss of these physical comforts had been one of their objections to the move. The question of individual offices was under discussion for several weeks; Mr. Watson explained to Mr. Weidlich, the head chemical engineer, that the limited available floor space would not permit all the engineers to have enclosed offices. Ultimately, however, he decided, in order to promote harmony, to give the chemical engineers their offices, even though the remainder of the engineers would have none.

In planning the engineering department Mr. Ryan adopted the accompanying organizational setup.



To accomplish this change, he had to break up the previous combination of the mechanical, construction, and maintenance engineers who had all worked under Mr. Merrill. In the new organization, Mr. Merrill kept

the construction and maintenance engineers and his former assistant, Mr. Cody, took charge of the mechanical engineers. Mr. Merrill did not receive this step favorably. He had been with the Haig company for nearly 20 years, he had actually hoped for the position of chief engineer, and he had been none too pleased with the introduction of Mr. Ryan as the chief engineer. Mr. Merrill, a self-made man, with very little formal education, had shown himself very capable in his operation of the mechanical, construction, and maintenance groups. Mr. Towne and Mr. Haig had not selected him for the position of chief engineer primarily because his background was limited and included very little chemical or industrial engineering.

After Mr. Ryan had accomplished the physical consolidation of the engineers and had defined the departmental organization (as shown by the chart), he set about to establish yardsticks for appraisal of results, to provide incentives for outstanding work, to institute informal channels of communication, and personally and forcefully to present the viewpoint of the combined engineering groups in any company discussions involving engineers. He held numerous dinner meetings at a Baltimore hotel, at which time the entire engineering force, under pleasant social surroundings, was able to discuss departmental engineering issues. Mr. Ryan made daily efforts to become familiar with his engineers, with the plant and its procedures, with key men in the research department and in the factory, and with the situations which arose causing confusion and conflict.

Mr. Ryan made a particular effort to reach close understanding with his four engineering group heads. He planned frequent conferences with Watson, Weidlich, Cody, and Merrill, in which they discussed in detail the problems that arose in their work, the areas which had formerly produced disagreement, the best procedures to follow, and other points involving the engineering department. In addition, Mr. Ryan dropped in to see these men at irregular intervals to discuss informally some phase of the department's operation. Mr. Ryan took the four men into his confidence on matters involving the department, particularly where departmental policy in regard to relations with other departments was concerned. In all these discussions, Mr. Ryan proved himself receptive to the ideas and opinions of the other engineers and was forceful in following up, outside the department, the decisions reached in conference within the department. By giving wide distribution to any letters, publications, or memoranda involving the department, he hoped to develop interest by all engineers in departmental activities. He made it clear by memoranda that achievement by engineers would be recognized within the department and made the basis for promotion, salary raises, and company recognition. Through his group leaders, he attempted to establish yardsticks for appraisal of results by clarifying the particular functions of each group and the basis for satisfactory performance. The group leaders were to

could not be measured in daily output but must be considered in the light of months and years of results.

Of the two development divisions under the associate director of the research department, the development research division likewise was composed of senior chemists utilizing a laboratory for scientific process development, while the chemical technology division included the chemical engineers who were studying process application to pilot plant and factory operation. The chemists in the former division were men similar to those described in the pure research divisions except that their work involved investigation of process rather than product discovery and analysis. They were required to work rather closely with the chemical engineering group, which applied their processes to pilot plant and finally to full-scale production. There was a tie between the chemists and the chemical engineers based on the similarity of their work, but, according to nontechnical executives, the chemists as a group felt that the pure scientific aspects of product and process development were of prime importance and that the chemical engineering and development work was of a less highly skilled and scientific nature.

The chemical engineers were men trained in college either in chemistry or chemical engineering. Some had doctors' degrees, and all were fundamentally interested in applied chemistry and physics. The function of this group was to work in conjunction with the process chemists, to become familiar with the process, and to streamline it for pilot plant and factory use. Various features were considered by this group, including plant and equipment design, yield rates, rate of production, and scheduling of flow. It was evident to company executives that they respected the senior chemists in the department and were generally tolerant of the group scientific pride felt by the chemists.

In the laboratories, in addition to the senior chemists, were numerous younger chemists and technicians. Most of these were college graduates, who visioned a future in chemistry similar to that enjoyed by the senior chemists. The attitude of devotion to science was prevalent throughout the laboratory groups.

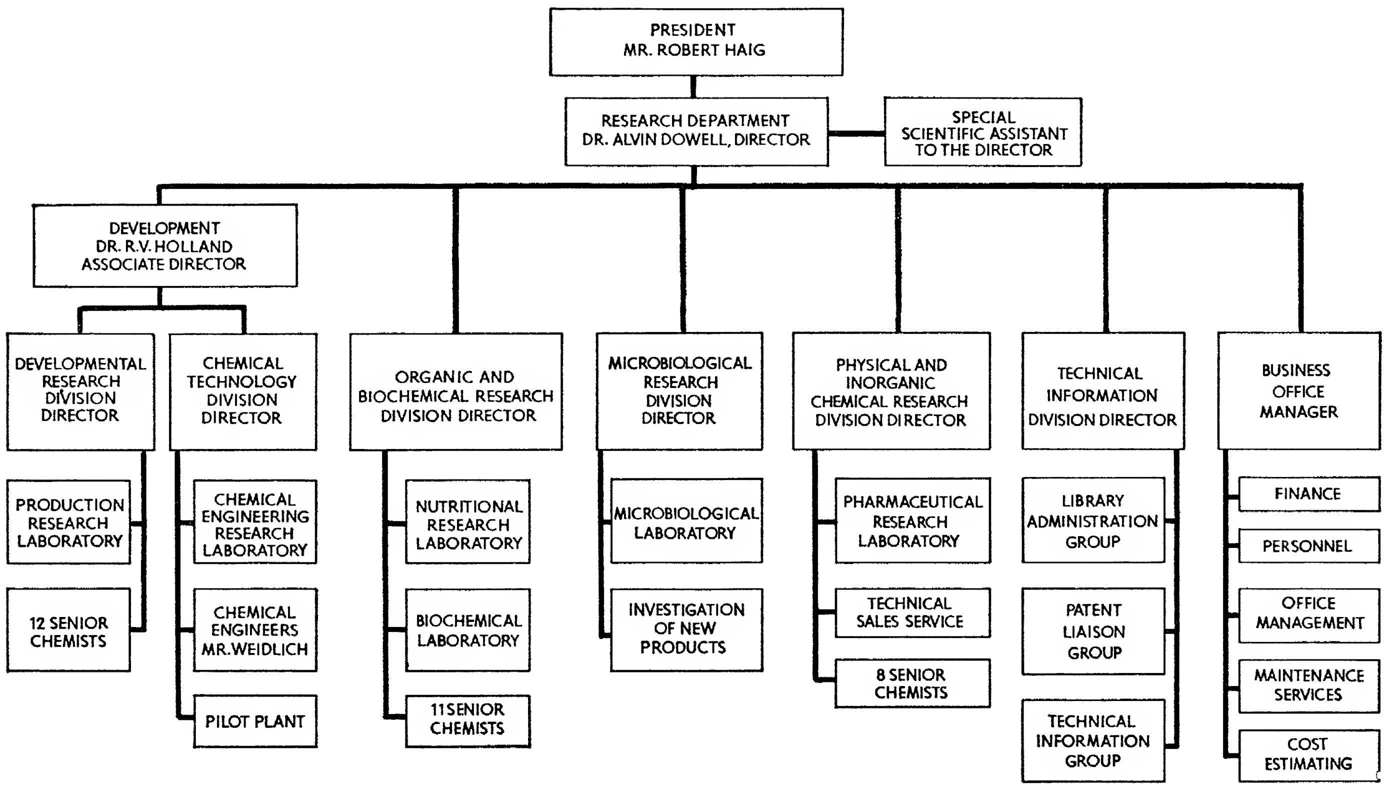
The information and business office divisions were purely service functions for the department, established to provide an ample library, patent information, technical information, and various office and control services.

The research department had been established and developed with large expenditures by the Haig company, and both the executives and the scientific men expressed their view that the facilities for both scientific work and comfortable office space were excellent. The laboratories were spacious, well lighted, well ventilated, and strictly modern; the offices and workrooms for chemists and engineers were large, comfortable, and well equipped; the service facilities were many; and the routine was conducive to uninterrupted study and investigation. The top management considered

Exhibit 2

HAIG CHEMICAL COMPANY

Research Department



assign engineers to particular projects and to follow up their efforts in carrying out the details of the project.

During this readjustment period, Mr. Ryan made special efforts to reach an understanding with Dr. Dowell on matters of responsibility, coordination, and authority between the two departments. The two men spent considerable time together in office meetings in the pilot plant and in the factory, discussing ways and means of placing new product development on a smooth functioning basis. At many of these discussions, Watson, Weidlich, Cody, Merrill, certain research department personnel, and various engineers were present. As problems arose that were difficult to settle on the spot, the engineers and chemists involved sometimes familiarized Dr. Dowell, Dr. Holland, and Mr. Ryan with the problem so that it might be settled by the formulation of a principle that would apply to other similar instances.

Dr. Dowell maintained responsibility for new products until they were being produced without difficulty in the factory. Mr. Ryan considered it the responsibility of his own department to provide all the functional services required by the research group. That is, the chemical engineers were to work in conjunction with the chemists on process and pilot plant operation; the mechanical engineers were to furnish all the machinery and equipment for pilot plant and factory operation; the industrial engineers were to take responsibility for all scheduling, time study, packaging and shipping; the construction and maintenance engineers were to provide plant space and plant layout for the process; and finally, the engineering department was to coordinate these engineering functions to provide maximum control, minimum duplication, and the greatest possible effectiveness in aiding the research department to place a new item in quantity production.

Although Dr. Dowell held the responsibility for new products, Mr. Ryan, as chief engineer, possessed in effect a power of veto over any step in the process which he felt was inadvisable from an engineering point of view. For instance, if the research department decided to install a process involving equipment and machinery which did not meet with the chief engineer's approval, for reasons of design, cost, or availability, Mr. Ryan discussed his objection with Dr. Dowell, attempted to reach an agreement, and, if necessary took the problem to the management committee for decision.

As time passed, it became evident that the engineers were all working together under one roof, were beginning to understand the attitudes of the other groups, and were responsible to one authority, Mr. Ryan, who was responsible for their promotions and recognition as well as for their activity. This caused a noticeable decrease in the friction which had existed. The chemical engineers became accustomed to their new surroundings, although they continued to spend a considerable amount of time in the research department, particularly with the development

chemists. They discovered that the move had not appreciably changed their relationships with the chemists and the research department. The chemical engineers developed individual friendships with men in the mechanical, industrial, and maintenance and construction groups, and found it easier to communicate informally with these groups. It became a common sight in the engineering office, as well as in the pilot plant and factory, to see members of several engineering groups discussing, informally, problems relating to certain features of a new product's development. Early in 1946, Mr. Weidlich, the head of the chemical engineers, who had so opposed the consolidation move, said to Mr. Merrill, "Bob, I'm actually surprised at how well this department is working out. It may be OK after all, and we are beginning to enjoy it here. There's no doubt that we are getting along better, and certainly it's easy to see an improvement in the procedure on new products. Hope we didn't give too much trouble in the beginning."

At the same time, an increased smoothness was noticeable in the manner in which the groups carried out their functions in the pilot plant and in the factory. Mr. Weidlich and his chemical engineers became more familiar with the other engineering personnel and were better able to call on the right group for assistance and to explain what was required. The chemical engineers dominated the pilot plant because of their knowledge of the process, but came to rely on the mechanical engineers, particularly for consultation on machinery design. Although Dr. Dowell and the research department still held the official responsibility for the product from beginning to end, the engineering groups actually took charge during the pilot plant and factory stages, and the research people began to serve mainly in an advisory capacity on the chemical aspects of the process.

Responsibility of Engineering and Research

By June, 1946, there had been no formal change in the responsibility of the research department for development of products from test tube to full-scale factory production. The topic was being discussed, however by engineers and by research personnel. Some of the engineers seemed to feel that the new engineering department should take over formally at pilot plant stage whereas the research men could see no reason why they should give up a responsibility that had been theirs since 1933 and without which they might not be fully capable of developing products and processes in accordance with their ideal standard. Since the volume of research work had increased so much and was of such great importance to the company, it was a moot question in the minds of many whether once again growth required a reshuffle of responsibilities. More specifically, the question was how responsibility for product development should be divided between

research, engineering, and the factory production staff. The views of several people regarding this question follow.

Under the operational scheme visualized by the engineers the new product development would be generally as follows: When the research lab discovered a new product and had conducted sufficient preliminary research to justify study of the item and process from a production point of view, the chemical engineers would begin to work with research chemists and familiarize themselves with the process. When decision was made to go ahead with production, the chemical engineers would make arrangements to institute the product in the pilot plant, and the mechanical and industrial engineers could assist where necessary. When the pilot plant operation was ready, the chemical engineers and chemists would jointly set the process in operation, but at this point the responsibility would shift because the major portion of the work load shifted from research to engineering. The chemists assigned by research to the project would continue to work with the chemical engineers. As the pilot plant operations became smooth and the product prepared for the factory, the research chemists would have less and less to do and could be reassigned by the research department, with the understanding that they were available for consultation on any problems that might arise on the later development of the problem.

The foregoing viewpoint was not shared generally by the research department. The research department had felt the pressure by the engineering department ever since its organization particularly as the engineering department increased in scope and effectiveness and bore more and more of the burden of carrying along the new product. Mr. Burnett, one of the research department division heads, explained his attitude by saying: "It's my opinion that research on a product discovered and developed in our research labs and destined for production in our factory is not completed until all the kinks are taken out of the factory process. To say that research is over when the product leaves the lab is ridiculous. The only reason the item leaves the lab is that we feel better equipped to carry out the advanced stages of the research in the pilot plant and factory. Our job is more than the discovery of the product and the formula; it is, in addition, the discovery of the best means of processing it. That is a job for a trained chemist, especially for the chemist who did the primary research on the product.

"We recognize the contribution of the engineers to the production processes, but the building of machinery and equipment, and the scheduling of work are only aids to the principal job of learning the most efficient way to produce the item. When we know these things, then it will be time for factory engineers to streamline their equipment and procedure for producing the item by the process that we've determined is most favorable. If, as is proposed, we turn a product over to the engineers when it

leaves the test-tube stage in the lab, I'm afraid that research on the product will be cut short, and although the speed of putting the item into full production may be increased, the final product and the final process will be inferior to what would be obtained if research were allowed to finish its job.

"Ever since the chemical engineers were transferred from research to the engineering department, we have experienced difficulty in coordinating the various groups involved in the several phases of new product development under Dr. Dowell. For instance, there has been noticeable a tendency on the part of the factory chemists to resist late changes in process, as suggested by the research chemists, after the manufacturing processes have been designed. We in research feel that we must have the privilege of improving product and process at any time during its development, particularly until the actual factory yield reaches the theoretical yield. This company has a splendid reputation now for research and the development of fine chemicals and drugs; we are establishing our name for quality products; and we are making a substantial contribution to medicine, science, and industry. To continue this progress we must insure that our products are as good as modern science can make them. That responsibility can be borne only by our research department and by our chemists and scientists. To deprive them of full responsibility for a new product until they are satisfied that it is as good as their facilities at present will make it, would be to endanger the control over quality, which is the basis for our success."

Chapter 15

SCIENTIFIC RESEARCH, INC.*

IN SEPTEMBER, 1957, about a year after he started work for Scientific Research, Inc., George Hurst realized that he was spending considerable time thinking about himself and his job. He was aware that his reactions to his work were complex and that he was uncertain about some of them, including whether he was presently in a situation which he would find congenial and in which he could be productive. His thoughts came to somewhat of a head one afternoon, when he happened to hear a remark that he took to be about himself made to Livingstone Kennedy, a technician who worked for him. The same afternoon he talked to Kennedy about their relationship. Some of their conversation and the background leading up to it are given in the following material.

Directly after graduating from college, George started to work for the electrical division of Scientific Research, Inc., whose main business was research connected with commercial applications in the field of communications (Exhibit 1). George had majored in physics and was employed mainly to do mathematical work on an important project to which the company's electrical division had committed most of its resources.

Scientific Research, Inc., had been in existence for some 30 years. Most of its efforts had been concentrated on what was called "conventional circuitry." A few years before George joined the firm, a large national corporation decided to move into a new field of communication design related to color transmission of an extremely complex nature in which little research had been done. The theoretical framework related to the design of the system had been partly worked out, but there was no evidence at the time that the system would be workable on a production basis. When the national corporation made the decision to go into the new field, it awarded the contract for the development of the system to Scientific Research, Inc., which immediately formed its electrical division to handle the project. At the start, the division was small and made up mainly of scientists, engineers, and technicians picked from other divisions of the company. Some new people were also hired at the time. After the division's first commercial successes, it grew rapidly as the scope of the work expanded. Several hundred technically and professionally trained people were employed at the time George was hired.

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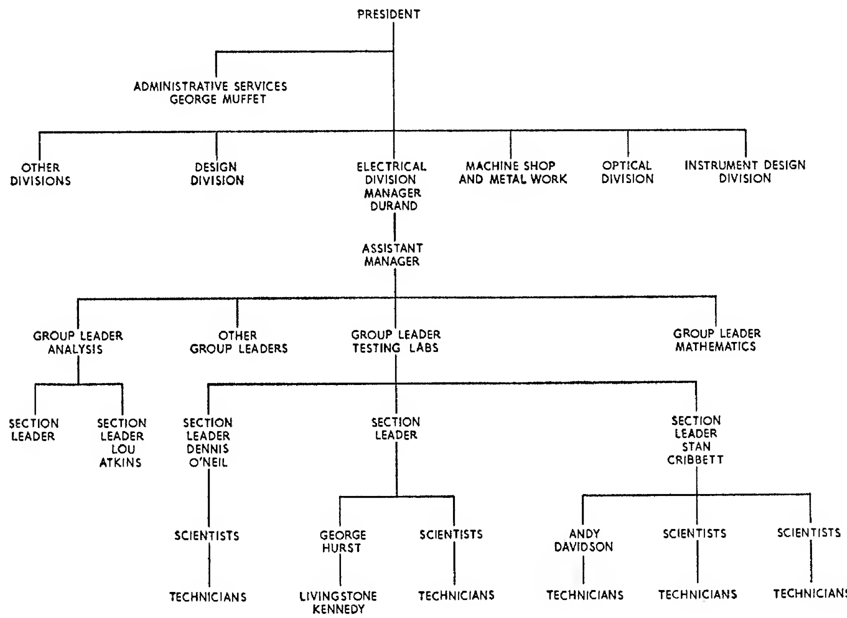
As George Hurst heard about it when he came to work, the decision to form the new division in effect revolutionized Scientific Research, Inc., which until then had been a quiet, almost unnoticed, firm. When George joined the division a dozen years later, people employed at the beginning of the project still recalled the pioneer period with nostalgia. One scientist, Dennis O'Neil, to whom George talked during his first days, spoke about it in the following terms.

Exhibit 1

SCIENTIFIC RESEARCH, INC.

PARTIAL ORGANIZATION CHART



DENNIS: I don't know what exactly has happened to us in the past year or so, but the enthusiasm we had at the beginning has almost died down. When I started, there was almost nothing here. We worked in what could be described as pitiful conditions, as far as scientific instrumentation was concerned. Things were arranged with old props and strings, but everybody was excited about them. We had a testing ground practically in the wilderness, because we needed lots of room without interferences, and we would have to spend weeks out there preparing the tests. The testing ground was situated in a hell of a place. There was only a very small village with an old hotel, where the only thing we could do was to drink an occasional beer. We worked 12 hours a day without overtime pay, because as scientists we were not entitled to any.

I still remember the first test we had when some of the officials of the

national corporation were present. Everyone was standing still and very nervous, while we were waiting for Dave to push the button. He did, and the thing flopped miserably. The officials were deadly serious; some of us were very sad. Dave laughed his head off and thought it was the funniest thing which could have happened. Somebody said that it was like the cartoons in the *Saturday Evening Post.*

Well, those were the pioneering days, and it seems as though a long, long time has elapsed since then. The group has been enlarged; regulations have been written down; the lab groups have been increased in size by a factor of four or five; the project is now at the experimental stage and is working; but gee . . . the enthusiasm is not there anymore. People are coming in and going out, and anybody with some ambition is dreaming about quitting or quitting as soon as a suitable job is available.

Lou Atkins, a scientist and section leader, who also talked with George Hurst when he first went to work, had done postgraduate work at MIT two years before; his promotion came a year and a half after he joined Scientific Research, Inc. Many at the company considered this a spectacular promotion, because they held that a university graduate could become a section leader only after four or five years of work as both junior and intermediate engineer. Lou talked with Hurst about life in the electrical division as follows:

LOU: In a way this is a very good place to work for somebody who is interested in research. Lots of freedom and autonomy in research and interesting projects to work on. But I feel somewhat dissatisfied. One thing is all the petty regulations. You have to come in at 9:00 o'clock in the morning; and if you are late, you report to "Mother Superior." I was talking about that with George Muffet, the head of the administration here. This guy thinks that ideas are produced like bearings. You start to be intelligent at 9:00 in the morning; and you stop somewhere around 5:00 in the afternoon. If you want to "buy" his way of thinking, you will find out that a lot of people don't start to be intelligent before 10:30, after the coffee break, and end around 4:30. Maybe someday he will start a piece-rate system for ideas. How can this man understand what it is all about? He came from the Army; he married a rich girl and thinks that a pair of shiny shoes are more important than a clever brain.

I kind of feel sorry about all this. In a way, I like my job. I have had a double increase in salary over the last two years, but I somehow feel that I am not going anywhere in terms of my life. I have an offer from a university to join their staff and I think more and more of doing it.

You know that we now have a lot of companies subcontracting with Scientific Research for the parts we need. The staffs of those companies have been built up very fast, especially in the last two years; their people know very little about the project. But when they are here, you have the impression that they are running the whole place. The other day we had a

meeting between some of them and the manager of our electrical division. Durand, the manager, has been buried in administration for five years. His technical knowledge has somewhat disappeared since then. This is perfectly normal, but the guy is so insecure that if any one of the engineers from a contracting firm says something, Durand tells him that he is right and we are wrong. He does not even give us a chance to speak.

The other day, one of the contractors' engineers said something about the fact that the FB-22 gauge could not work. Gee, we had it working for a year and a half in the lab, and those guys tried it once and it did not work. What the hell, when your boss shows in front of the whole contractors' group that he does not have any confidence in his men! And on such silly, well-known, little things. Durand is really extremely insecure and is afraid of what outside contractors may say to his boss.

In contrast to his present situation, George remembered that he had felt his college years to be happy ones. Though the schedule of work was heavy, he had felt that he had considerable freedom in deciding what subjects he would work on. He did not have any problems with grades, with the result, particularly in his senior year, that he could forget about what he considered second-rate courses and subjects and concentrate on what was most exciting to him. He remembered with pleasure some of the informal discussions his group of students had had with some of their professors in their challenging courses. He derived considerable satisfaction from solving mathematical problems and in finding vigorous and concise symbols to express solutions. Examinations were held often, and he enjoyed having the good grades he received. All this, he realized as he thought back over it, gave him a competitive spirit, a chance to evaluate himself in comparison with other students, and the approval of his teachers. He had felt at home in the department of physics, where proficiency and speed in mathematics and a strong appreciation for background knowledge in the arts and philosophy were both usual and appreciated. He remembered that these kinds of rewards for intellectual accomplishment were what he hoped he would receive from the job he would hold after graduation. He turned down many job offers from industry, which did not seem to him to offer this type of challenge.

The first day that George was at the electrical division of Scientific Research, Inc., he met the section leader under whose direction he was to work and also two technicians who were to work for him on a part-time basis. Though he had acquired some reputation at the university for his mathematical ability and was hired to work on mathematical problems, his first job was a laboratory job, to study the specifications of a new gauge of unusual design for advanced tests of the division's products. During the first few days, George talked mainly with the six technicians who worked in the two adjacent labs under the same section leader. He got to know especially Livingstone Kennedy, one of the technicians

assigned to him, whom everybody called "Living," and who was the more outspoken of the technicians. It became apparent that he was somewhat of a popular figure, as many people stopped in the lab during the day to joke and chat with him. He was 35 years old and had been working for Scientific Research for about 12 years. Sometime previous to George's joining the division, he had been a technician for the two men who were currently the heads of the division.

Living offered himself to George to act as a guide for a tour of the labs and offices, which were located on separate floors of the division's building. As they visited the supply stores for lab apparatus, the instrument stores—from which high-priced standardized equipment for laboratory use could be borrowed—and the machine shops, George noticed that Living had friends everywhere. He teased people a lot and was teased in return. He introduced George to the men in the supply stores saying, "This is a young university graduate who is joining our laboratory. He is going to work with me and do research. You may find him looking for a broomstick sometimes. I hope you will help him."

Later George learned from Living that "researching a broomstick" was the standard joke among the technicians. When a scientist was looking for something and could not find it, the technicians would say, "This man is one of the best researchers here. Actually, he is researching a broomstick." Another standard joke about the scientists among the technicians was to say of one who was disliked, "This man does research which is so confidential that they did not even tell him what he was to research. Ever since he has been here, he has been trying to find out what his job is. These kinds of projects are given only to the brightest people."

As his first week passed, George learned more and more about the customs of the labs from Living, who spoke to him as follows about some of them.

LIVING: If you want to get something done around here, you will find it pretty tough, unless you know the right people. Every time you want to get a nonstandard piece of equipment, you have to fill in a form in 12 copies; then you scrap 11 copies and put the other in your files. There are other ways of doing it, like going through formal channels, but it does not go faster. If you really need something, tell me and I will get it for you.

Later on, George and Living were talking about Andy Davidson, a scientist working in another section, who had graduated from the same university as George. Living spoke as follows.

LIVING: Here is an s.o.b. There is nothing that guy would not do to get ahead. He is always "sucking" the boss, and he despises the technicians. He is a hell of a guy to work for. He yells at you constantly. He is always

pushing. He thinks he is better than the rest of us because he went to a university. He is the type of guy who learns from you while telling you that you are wrong, that you don't understand. Then when you tell him how it works, he picks it up and explains it to the boss as if he had discovered it himself. If the boss comes in the lab and seems to be dissatisfied with something, he agrees with the boss and says that he had already asked for something to be done about it, but that the technicians did not do it.

He thinks he is better than the rest of us because he went to a university, but look at it where he is now. Nobody wants to work under him. Even the boss doesn't like him, and he works in the barracks down there where nobody else will go. The bosses like a man like him because they make him do the dirty work, but he does not get promoted. Every time he has to get help from other people, they get in his way and make it a purpose to delay what he is asking for. Everybody dislikes him; he will never get anywhere.

George took these and similar comments as indicating that life would be a lot easier if he recognized the importance of the technicians, most of whom, like Living Kennedy, had spent many years at Scientific Research. They had seen a lot of people come in and go out. Previously they had worked closely with the heads of the organization; now they hoped mostly for security. Most of them had been locally hired; they had families and houses in the nearby city and saw little opportunity to move to other firms. On the other hand, the scientists in most cases had very little seniority with the company. Most of them had come after the growth of the electrical division; turnover among them was high.

At the time, in many parts of the United States, a tremendous development had taken place in all fields of communications; in some regions an inflow of scientists from Europe had helped fill the gap in the demand for trained people. Most of them joined small firms like Scientific Research which did not yet have a systematic program for canvassing universities and which could not spare the money to train young university graduates. In many cases these men had acquired long experience in Europe and looked at Scientific Research as a steppingstone to larger companies. They came from many different countries, had little social experience in common, and were looking for bigger jobs. Most of them were individualists, some strongly oriented to scientific values, while others looked mainly to administrative work for promotion. Men with all these backgrounds worked at Scientific Research. A small number of young graduates from local universities completed the group of scientists at the company.

When George started to work on his project, he made it a point to do the calculations and to write the reports on a small table in one corner of his lab. The other scientists usually spent as little time as possible in the labs and did most of their work in their offices upstairs. For the first few

months, George felt his work went relatively well. He became friendly with his group of technicians and discovered that it was sometimes easy to find a piece of apparatus that others had difficulty in locating.

As time went by, though, George began to feel dissatisfied with his work. His laboratory work gave inconclusive results, and it became impossible for him to give clear-cut answers to the questions being asked. Meanwhile, he had started another experiment on another type of gauge, the operation of which was even more complex. It was not possible to build a clarifying theory about its functioning, and again he faced the difficulty of answering the test questions clearly. The gauge had been studied by many people; publications showed opposite and contradictory results. As George felt more and more his basic weakness in lab work, he began to spend time upstairs helping people in the section with mathematical problems.

One day a scientist who worked in the same section as Hurst told him, "You know, George, I was amazed by your handling of the Fourier series [a mathematical series] the other day. Up to that time I thought you were only a technician." George felt hurt by this statement. He felt more and more that because of his behavior, he was "missing the boat." Moreover, because it was almost impossible for his section leader to evaluate his productivity and for himself to compare his work with that of others, he had a strong feeling of being isolated. He also felt a lack of the competition he had enjoyed at the university. One day he received a phone call from Stan Cribbett, a section leader, in one of the testing labs asking him, as he often did, to prepare a sample for testing as soon as possible. George replied that he had started some other work and could not do it right away, but would as soon as possible. Cribbett told him that it was needed in a hurry and that he should do it as soon as possible.

The next day George brought the work Cribbett had asked for to the lab and the following conversation took place.

STAN: What in the hell are you doing here?

GEORGE: I came with the work you asked for.

STAN: What work?

GEORGE: Didn't you phone yesterday?

STAN: Oh, yes, but this was only for next month.

GEORGE: But you said it was urgent.

STAN: Sure, it is urgent. But we don't need it before next month. It is not that urgent.

GEORGE: Ah!

STAN: George, how long have you been working here?

GEORGE: Six months.

STAN: You'll learn.

George Hurst left the lab very dissatisfied. It seemed to him that there was no point in doing a good job, if the people involved were not interested in what he was doing. He tried more and more to build up his rela-

tions with his section leader. He helped the latter solve some theoretical problems which presented unusual difficulties, and he spent less and less time in the lab. One afternoon, he spent some time discussing with his section leader the second of the two gauges he was working on. Both of them thought that some basic assumptions about the gauge were false and that an oscilloscope trace in response to a signal would show it. There was to be a meeting of the testing group a few weeks later, and they hoped to be able to show the whole group at that time that the basic assumptions about the gauge were false and misleading.

George went to the lab the next morning and told Living that he would like to get photographs of an oscilloscope trace of the gauge under certain conditions and that this was urgent. Living explained to him that he had some other work to do. George felt aggrieved and told Living that this new work should have the highest priority.

That afternoon George came down to the lab and saw that only half of the apparatus was ready. Living was sitting on a table doing what seemed to George nothing related to the job he had assigned him. George felt irritated and spoke as follows.

GEORGE: Living, I told you this had to be done in a hurry.

LIVING: I am doing it as fast as I can.

GEORGE: Only half of the apparatus is ready.

LIVING: I have to wait for the storeroom to call me about when they will have a camera.

GEORGE: Do you mean to say that no cameras are available now?

LIVING: Yes.

The next morning, when George went to the lab, he saw Living sitting on the table again.

GEORGE: How are the photographs, Living?

LIVING: I can't make them. I don't know at what speed you want the 'scope to work.

GEORGE: Why didn't you ask me? Operate it at 3 milliseconds. (*Living walked slowly to the oscilloscope.*)

During the afternoon, Living came to George's office, threw some pictures on the table, and said in a rough voice, "Does this satisfy you?" Later when George looked at the pictures, he found that they showed a clear trace with none of the side effects he had expected. He felt sure that there were side effects, but he saw that the problem of showing them would be more difficult than he had thought. He felt somewhat frustrated that Living did not seem able to find them.

It became apparent after some days that in spite of all George could do to hurry the tests along, the photographic traces would not be ready in time for the meeting. One day during this period when he came down to the lab, Living spoke to him about a recent visit he had made to Durand, the manager of the electrical division.

LIVING: I am fed up.

GEORGE: What's wrong, Living?

LIVING: I went to ask Durand if I could take two days off for a trip I had to make. Do you know what he said?

GEORGE: What?

LIVING: Well, the first thing he said was "No." But this is not why I am mad. He started to make a whole speech about my work and said that I was becoming lazy, that my work was not satisfactory. I questioned him about the last six months, and he said that during the last six months my behavior has been slightly better. He told me that being lazy is not a way to get somewhere and to advance, that he had to work like hell to get his degree in engineering and his doctorate in science. I told him that I would have liked to have my degree also, but that I had to take care of and support my mother and my younger brother; that my father was not polite enough to ask my permission to die. He just died and left me with three brothers and my mother to provide for.

GEORGE: Sometimes I think that Durand is not intelligent.

LIVING: And that is not all. I went to the doctor last week, and he told me to take it easy because I have a heart condition. He told me I had better take it easy.

GEORGE: I am sorry, Living. That is too bad. I hope you will get better.

After this whole series of episodes with the second gauge, George's dissatisfaction with his job increased. Not only did he feel inadequate in the lab, but it seemed to him the technicians did not seem either able or willing to do a good job for him. He had a vague sentiment that more than a heart condition was needed to explain Living's behavior.

One day, as George was passing the lab, he heard another technician say to Living: "Living, how do you handle your little dictator these days?" As George walked on, he did not question for a minute to whom the remark referred: he was sure it applied to him. He felt worse than ever. He thought he had been friendly enough with the technicians and that he had listened sympathetically to their complaints about the scientists and management. As he thought back over the technicians' comments about the scientists, he became more and more convinced that they were unjust; that the technicians—including Living—were bitter, lazy, and complaining all the time; and that if no work was done, it was because of them. They came to get their pay checks and did not want to work. They resisted everything. He remembered that the technicians, when together, talked about their houses and their families, never about their work.

As he thought over all this, his feelings became stronger against the technicians. After a while he began to wonder why he had been angered but not surprised by the technician's remark and why he immediately applied it to himself, as if he knew that it were true. After an hour or so,

spent mostly in thinking along such lines, he decided that a way to resolve some of his questions was to ask Living what was going on. He came back to the lab and spoke to Living as follows.

GEORGE: Living, I have the impression that something has gone wrong between us. Maybe I have made mistakes. Will you tell me? . . . [*Pause.*]

LIVING: How can I say anything?

GEORGE: Come on, Living. Shoot . . . [*Pause.*]

LIVING: You really want to know? [*Pause.*] . . . I will tell you what is the matter. You push all the time. You never give the other fellow a chance. You want to run things and you don't give a damn for us. You come down here, and you tell me: "Do this." "Do that." "How come this is not ready yet?" "I want that for this afternoon." You don't know how to solve the problem, so you push us all the time.

GEORGE: You think I am like Andy Davidson?

LIVING: Like Andy Davidson, Stan Cribbett, and the rest of them. At the beginning you were not that bad, but now—you think you know enough to push us as much as you want. You are like the others. At the beginning you didn't know very much, so you needed us to learn about the organization. You were polite. But lately, more and more you think you know more than we do. You drive people around. You don't care about us any more, because you know the organization well enough to go around by yourself. You seem to think we are here to be your servants, so that's how you use us.

GEORGE: I am glad that we have talked about this. I had the feeling that something was going wrong, but I did not know what. I was nervous about the project myself, and I can see how I may have given the impression that I wanted to push you.

LIVING: Just yesterday, you came in and asked me bluntly why the job was not done. You did not discuss it with me then or previously. You just told me, "I need some oscilloscope traces this afternoon." You did not even tell me how I was to get them. When you came here a year ago, this was not how you handled things. We used to discuss them, and we would argue about how we would do something.

GEORGE: You liked that.

Chapter 16

THE UTILITY POWER COMPANY, PART I*

MEMBERS of engineering management at the Utility Power Company were, in 1959, searching for a method to increase the work capacity of the distribution section. Pressures for expansion of the company's electrical distribution system were increasing rapidly, calling for a marked improvement in the speed with which construction and renovation designs were made available to field construction crews.

The Utility Power Company held a franchise covering a large area of the midwestern part of the United States. Its distribution territory was divided into four regions, each responsible for the construction, maintenance, and operation of power substations, transmission lines, and auxiliary equipment, as well as for commercial activities related to customer service. The regional offices relied upon a central engineering group, the distribution section, for the design and cost information required in expanding their service facilities.

The distribution section was housed at company headquarters, located approximately 150 miles from any one of the regional offices. Two teams, in each an engineer and a draftsman, reported to the assistant section manager. Their exclusive assignment was to design and estimate costs for power substations throughout the company's franchise area. The section manager directly supervised four teams, each composed of an engineering supervisor, three engineers and two draftsmen. Each of these four teams was responsible for designs and cost estimates for all other equipment requested by a particular distribution region. The composition of the regional teams varied somewhat from time to time, depending upon the work load called for by the various regional offices.

The distribution engineering teams had each developed close, friendly working relationships over the years. Engineers and draftsmen were salaried and nonunion. The men of each team had considerable opportunity to work closely together. It was the custom for all members of a team working on a particular engineering problem to travel together for several days at a time, surveying some portion of a region's extensive territory. On their survey trips, and at the home office, each draftsman felt himself to be as closely integrated into his team as were engineers. Although there

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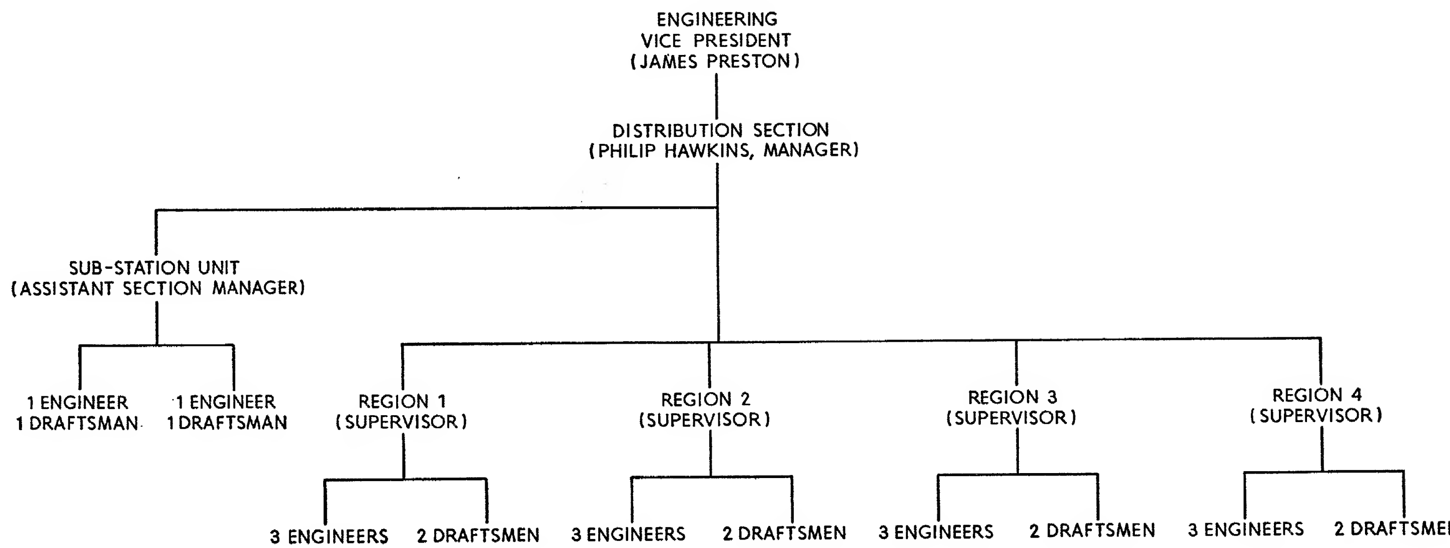
Exhibit 1

THE UTILITY POWER COMPANY (A)



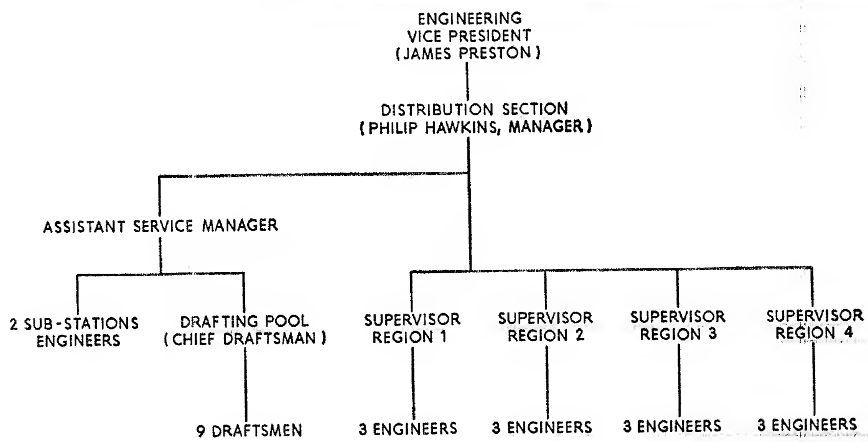
were obvious distinctions of technical status (e.g., each supervisor had his own office, the engineers of each team were located together, and the draftsmen of all teams worked in one large room), there appeared to be little formal barrier between team members as they worked together. Even across team lines, there appeared to be a great deal of informal, friendly interchange of information.

The draftsmen of each team, in addition to routine drafting duties, were frequently assigned those elementary design and estimating problems of which their engineering colleagues found them capable. The availability of such work made technical and salary advancement for the

Exhibit 2

THE UTILITY POWER COMPANY

PROPOSED ORGANIZATION OF THE DISTRIBUTION SECTION



draftsmen a readily attainable goal. Furthermore, some of the engineers showed considerable interest in helping draftsmen, particularly those who were enrolled in correspondence schools or night courses, to improve their technical skills.

The distribution section had operated in the foregoing manner for many years. Since World War II, a number of engineers and a few draftsmen had been hired as replacements and as increases to the overall engineering capacity of the section. In spite of these additions, however, a backlog of essential projects from the field had steadily mounted. Repeated efforts to obtain qualified new men, particularly draftsmen, had met with little success. In 1959, Philip Hawkins, the distribution section manager, proposed to his superior, James Preston, that the distribution section be reorganized along more efficient and more highly productive lines.

Hawkins' plan entailed the establishment of a draftsmen's "pool." Under this proposal all draftsmen would be removed from team affiliation, to be placed under the supervision of a chief draftsman. Exhibit 2 shows

the proposed section organization. The new supervisor would assign to draftsmen the work received from engineers according to his estimate of the individual capacities of his men and by reference to a predetermined priority schedule. Hawkins predicted that the new system would increase available drafting time and, through greater specialization, would enhance the productivity of engineers and draftsmen, alike.

The proposed system was to have several additional benefits. It would remove the causes for the "sloppy setup" in controlling supplies and operating printing and duplicating machinery. Stocks of drafting supplies had traditionally been haphazardly maintained, with occasional depletions of materials occurring unnoticed. Similarly, reproducing machinery had not been giving good service because, it was believed, each draftsman was operating the equipment to secure his own print requirements. The pool arrangement would make it possible to assign to the chief draftsman a girl in charge of the stockroom and the duplicating and printing equipment.

Mr. Preston agreed to study Hawkins' ideas in preparation for further discussions a few days hence.

Chapter 16 (Continued)

*THE UTILITY POWER COMPANY, PART II**

After several discussions of a plan to reorganize the distribution section, the engineering vice president agreed with the section manager that the institution of a drafting pool would improve the section's capacity to meet its work schedule. The following implementary plan was considered by both men to be essential.

Key to the success of the pool would be the choice of a chief draftsman. It was agreed that the job would require a man with obviously superior technical ability. Since the new supervisor would have to be chosen from the ranks of the present drafting group (no outsider being available for consideration), he would have to have won the respect of all the men who were to report to him. One of the draftsmen, Roger Manson, was believed to be "head and shoulders" above the rest. Hawkins reported that Manson was working hard to increase his drafting skill, that he was more objective and aggressive in his work than others and that he had been given more advanced engineering work than any other draftsman. Manson had been with the distribution section for six years, somewhat less than had the majority of draftsmen. Previously, he had spent five years as a Utility Power Company substation maintenance man. He was 33 years old, about average for the drafting group. While supervisory experience would have been desirable, none of the draftsmen possessed that qualification.

It was decided, first, to advise Manson in confidence of the new plan in order to ascertain his willingness to accept the supervisory position. Manson would be warned that he would have to rely upon the leadership which the respect for his ability would generate, rather than upon his formal position. In no case would "heavy handedness" be advisable.

Assuming that Manson would accept the new job, Hawkins was to call a meeting of the entire section, including clerks and stenographers, as well as engineers and draftsmen. At this meeting he would carefully explain the nature of the change and the reasons for it. The new chief draftsman would be introduced and his duties outlined. Questions would be solicited and discussed until it appeared that the change was well understood. On the presumption that this meeting would proceed successfully, the new organization would be put into effect the next day.

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Chapter 16 (Continued)

THE UTILITY POWER COMPANY, PART III*

Early in October, 1959, Philip Hawkins, manager of the distribution section of Utility Power's engineering department, in a confidential meeting proposed to Roger Manson, draftsman in the section, that Manson assume the duties of a new section post, that of chief draftsman. After listening to Hawkins' description of the job and the authority limits within which he would be expected to function, he eagerly accepted the position. Over the next week the two men met several times to plan for the organization of a drafting pool. Both men were enthusiastic about the potentialities of the new plan.

On October 12, a meeting of the full section was called, the plan was explained in detail, and the chief draftsman's appointment was announced. A number of questions arose and were discussed. By the end of the meeting everyone involved appeared to understand the change and its implications for each individual. On October 13, the plan was put into effect.

During the remainder of October and through November, the pool system seemed to be shaking down well. There were minor grumblings now and again from engineers who believed a job of theirs had not been given sufficient priority by the chief draftsman. But, in the drafting room, cooperation seemed high and the production of drawings was much improved. The chief draftsman had introduced several new techniques for correcting and reproducing drawings, which had significantly reduced the need for tedious corrections and redrawing. The supply situation was brought under control, files were straightened out, and the appearance of the drafting room, in general, was radically improved.

The persistence of one custom, a holdover from the team system, seemed to be reducing the potential for further increases in the production of drawings, however. Engineers continued to take former teammate draftsmen on field survey trips. Draftsmen on these trips, which lasted several days, took notes and even carried some of the less complex jobs through the cost estimating stage of design upon their return to the section office. In consequence, these men were out of the chief draftsman's control for a week or more at a time. Despite the problems which this

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custom posed for the chief draftsman's effective scheduling of work, production of the pool was more than satisfactory.

Then, through December and the early months of 1960, Philip Hawkins began to observe signs of increasing discontent. He heard from some of the engineers that their draftsmen friends had confided in them their growing impatience with the chief draftsman. They reported such comments as, "I used to like that guy, but now that he's got his new job he thinks he can push any kind of petty job on me and expect me to thank him for it." Others remarked, "The work just isn't interesting like it used to be. You never know what's going on with a job. You draw a piece of it and it gets whipped away from you before the ink is dry."

Hawkins also observed a mounting friction between the engineers and the chief draftsman. Manson and Hawkins had agreed that the chief draftsman would have to know a good deal about each job in order to schedule drafting work properly. Particularly, Manson felt that this knowledge was critical for his ability to check drawings, as he always did, before returning them to the engineer for final authorization. However, several of the engineers referred to Manson's efforts as "interference." They remarked in Hawkins' presence that they could not help but be impatient with having to "go through the whole story again" for Manson's benefit. They talked as though Manson stood between them and the draftsman with whom they were "anxious to get down to cases."

In February, 1960, Hawkins began to notice that production of design for field construction crews was falling off. The engineering supervisors reported to Hawkins that their engineers and a number of draftsmen were highly discontented. The supervisors warned that something had to be done before a full-fledged explosion occurred. One of them even suggested that the pool idea be abandoned and that the section revert to its team system of operation.

Chapter 16 (Continued)

*THE UTILITY POWER COMPANY, PART IV**

By the middle of March, 1960, Philip Hawkins, manager of the distribution section of the engineering department, was convinced that some immediate action would have to be taken to reverse the productivity trend of his section. The situation had been alleviated for a time by a change in the policy for capital plant additions. Formerly, every addition in excess of $1,500 net estimated cost was the responsibility of his section. That amount had been raised to $2,500, thus eliminating about three fourths of that minor estimating work, which draftsmen were capable of handling on their own. However, Hawkins felt sure that, with increasing labor and materials costs, many of these small jobs would soon be back in his jurisdiction.

Hawkins discussed the worsening situation with the chief draftsman, Roger Manson. Manson, however, was convinced that the new capital additions policy would provide the relief needed to make the pool system operable. Nevertheless, Hawkins felt that the pool idea had proven itself unsuccessful, and he determined to abandon it in favor of a modified team system.

After consultation with the engineering supervisors, Hawkins decided to retain the centralized supply and reproduction facilities in the charge of a girl who would report to one of the substation design engineers. Otherwise, the pool idea was to disappear altogether. The teams were to be reconstituted, with the express provision that each supervisor was to arrange for the loan of draftsmen between teams whenever the need arose. Whenever these mutual arrangements failed, the matter was to be brought to the attention of the assistant manager.

Manson was to be reincorporated, at his present salary level, into one of the teams. Hawkins, in discussion with Manson, stressed that this action was not to be construed as a reprimand of the chief draftsman. In fact, it was simply a response to an unworkable organization which had been initiated by the section manager. Hawkins told Manson that he realized the ex-chief draftsman would probably feel awkward at first about his new status, but that he should do his best to readjust to it. Simultaneously, Hawkins met alone with the engineering supervisor in whose group

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Manson would work. He received assurances from the supervisor that the pool system and Manson's position in it were closed issues.

Shortly thereafter a general meeting of the section was called. Hawkins explained that the pool idea had not succeeded, as everyone knew, that its failure was no one's fault, except possibly his own, and that a modified team arrangement, as previously described, would go into effect immediately. The assignment of draftsmen to the teams was also announced at this time.

Chapter 16 (Concluded)

*THE UTILITY POWER COMPANY, PART V**

IN DECEMBER, 1960, Philip Hawkins reported that the revised team system of operation in his section had proven successful. By means of several follow-up efforts on Hawkins' part, excellent cooperation between teams had been achieved, and draftsmen were often sent on temporary loan to other teams whenever drafting commitments warranted. There had been some anxiety at first about the readjustment of Manson but, after several months, he appeared to be reasonably well integrated into his team. The productivity of the section seemed to Hawkins to be at an all-time high.

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Chapter 17

BOB KNOWLTON*

Bob Knowlton was sitting alone in the conference room of the laboratory. The rest of the group had gone. One of the secretaries had stopped and talked for a while about her husband's coming induction into the army, and had finally left. Bob, alone in the laboratory, slid a little further down in his chair, looking with satisfaction at the results of the first test run of the new photon unit.

He liked to stay after the others had gone. His appointment as project head was still new enough to give him a deep sense of pleasure. His eyes were on the graphs before him, but in his mind he could hear Dr. Jerrold, the project head, saying again, "There's one thing about this place that you can bank on. The sky is the limit for a man who can produce!" Knowlton felt again the tingle of happiness and embarrassment. Well, dammit, he said to himself, he had produced. He wasn't kidding anybody. He had come to the Simmons Laboratories two years ago. During a routine testing of some rejected Clanson components he had stumbled on the idea of the photon correlator, and the rest just happened. Jerrold had been enthusiastic; a separate project had been set up for further research and development of the device, and he had gotten the job of running it. The whole sequence of events still seemed a little miraculous to Knowlton.

He shrugged out of the reverie and bent determinedly over the sheets when he heard someone come into the room behind him. He looked up expectantly. Jerrold often stayed late himself, and now and then dropped in for a chat. This always made the day's end especially pleasant for Bob. It wasn't Jerrold. The man who had come in was a stranger. He was tall, thin, and rather dark. He wore steel-rimmed glasses and had on a very wide leather belt with a large brass buckle. Lucy remarked later that it was the kind of belt the Pilgrims must have worn.

The stranger smiled and introduced himself. "I'm Simon Fester. Are you Bob Knowlton?" Bob said yes, and they shook hands. "Doctor Jerrold said I might find you in. We were talking about your work, and

* This case was prepared by Professors Alex Bavelas, A. H. Rubinstein, and H. A. Shepherd for courses in Management of Research and Development conducted at the School of Industrial Management, Massachusetts Institute of Technology, Cambridge, Mass., and is used with their permission.

I'm very much interested in what you are doing." Bob waved to a chair.

Fester didn't seem to belong in any of the standard categories of visitors: customer, visiting fireman, stockholder. Bob pointed to the sheets on the table. "There are the preliminary results of a test we're running. We've got a new gadget by the tail and we're trying to understand it. It's not finished, but I can show you the section that we're testing."

He stood up, but Fester was deep in the graphs. After a moment, he looked up with an odd grin, "These look like plots of a Jennings surface. I've been playing around with some autocorrelation functions of surfaces —you know that stuff." Bob, who had no idea what he was referring to, grinned back and nodded, and immediately felt uncomfortable. "Let me show you the monster," he said, and led the way to the workroom.

After Fester left, Knowlton slowly put the graphs away, feeling vaguely annoyed. Then, as if he had made a decision, he quickly locked up and took the long way out so that he would pass Jerrold's office. But the office was locked. Knowlton wondered whether Jerrold and Fester had left together.

The next morning, Knowlton dropped into Jerrold's office, mentioned that he had talked with Fester, and asked who he was.

"Sit down for a minute," Jerrold said. "I want to talk to you about him. What do you think of him?" Knowlton replied truthfully that he thought Fester was very bright and probably very competent. Jerrold looked pleased.

"We're taking him on," he said. "He's had a very good background in a number of laboratories, and he seems to have ideas about the problems we're tackling here." Knowlton nodded in agreement, instantly wishing that Fester would not be placed with him.

"I don't know yet where he will finally land," Jerrold continued, "but he seems interested in what you are doing. I thought he might spend a little time with you by way of getting started." Knowlton nodded thoughtfully. "If his interest in your work continues, you can add him to your group."

"Well, he seemed to have some good ideas even without knowing exactly what we are doing," Knowlton answered. "I hope he stays; we'd be glad to have him."

Knowlton walked back to the lab with mixed feelings. He told himself that Fester would be good for the group. He was no dunce, he'd produce. Knowlton thought again of Jerrold's promise when he had promoted him—"The man who produces gets ahead in this outfit." The words seemed to carry the overtones of a threat now.

The next day, Fester didn't appear until mid-afternoon. He explained that he had had a long lunch with Jerrold, discussing his place in the lab. "Yes," said Knowlton, "I talked with Jerry this morning about it, and we both thought you might work with us for awhile."

Fester smiled in the same knowing way that he had smiled when he mentioned the Jennings surfaces. "I'd like to," he said.

Knowlton introduced Fester to the other members of the lab. Fester and Link, the mathematician of the group, hit it off well together, and spent the rest of the afternoon discussing a method of analysis of patterns that Link had been worrying over for the last month.

It was 6:30 when Knowlton finally left the lab that night. He had waited almost eagerly for the end of the day to come—when they would all be gone and he could sit in the quiet rooms, relax, and think it over. "Think what over?" he asked himself. He didn't know. Shortly after 5:00 P.M. they had all gone except Fester, and what followed was almost a duel. Knowlton was annoyed that he was being cheated out of his quiet period, and finally resentfully determined that Fester should leave first.

Fester was sitting at the conference table reading, and Knowlton was sitting at his desk in the little glass-enclosed cubby that he used during the day when he needed to be undisturbed. Fester had gotten the last year's progress reports out and was studying them carefully. The time dragged. Knowlton doodled on a pad, the tension growing inside him. What the hell did Fester think he was going to find in the reports?

Knowlton finally gave up, and they left the lab together. Fester took several of the reports with him to study in the evening. Knowlton asked him if he thought the reports gave a clear picture of the lab's activities.

"They're excellent," Fester answered with obvious sincerity. "They're not only good reports; what they report is damn good, too!" Knowlton was surprised at the relief he felt, and grew almost jovial as he said good-night.

Driving home, Knowlton felt more optimistic about Fester's presence in the lab. He had never fully understood the analysis that Link was attempting. If there was anything wrong with Link's approach, Fester would probably spot it. "And if I'm any judge," he murmured, "he won't be especially diplomatic about it."

He described Fester to his wife, who was amused by the broad leather belt and the brass buckle.

"It's the kind of belt that Pilgrims must have worn," she laughed.

"I'm not worried about how he holds his pants up," he laughed with her. "I'm afraid that he's the kind that just has to make like a genius twice each day. And that can be pretty rough on the group."

Knowlton had been asleep for several hours when he was jerked awake by the telephone. He realized it had rung several times. He swung off the bed muttering about damn fools and telephones. It was Fester. Without any excuses, apparently oblivious of the time, he plunged into an excited recital of how Link's patterning problem could be solved.

Knowlton covered the mouthpiece to answer his wife's stage-whispered "Who is it?" "It's the genius," replied Knowlton.

Fester, completely ignored the fact that it was 2:00 in the morning, proceeded in a very excited way to start in the middle of an explanation of a completely new approach to certain of the photon lab problems that he had stumbled on while analyzing past experiments. Knowlton managed to

put some enthusiasm in his own voice and stood there, half-dazed and very uncomfortable, listening to Fester talk endlessly about what he had discovered. It was probably not only a new approach, but also an analysis which showed the inherent weakness of the previous experiment and how experimentation along that line would certainly have been inconclusive. The following day Knowlton spent the entire morning with Fester and Link, the mathematician, the morning meeting having been called off so that Fester's work of the previous night could be gone over intensively. Fester was very anxious that this be done, and Knowlton was not too unhappy to call the meeting off for reasons of his own.

For the next several days, Fester sat in the back office that had been turned over to him and did nothing but read the progress reports of the work that had been done in the last six months. Knowlton caught himself feeling apprehensive about the reaction that Fester might have to some of his work. He was a little surprised at his own feelings. He had always been proud—although he had put on a convincingly modest face—he had been proud of the way in which new ground in the study of photon measuring devices had been broken in his group. Now he wasn't sure, and it seemed to him that Fester might easily show that the line of research they had been following was unsound or even unimaginative.

The next morning, as was the custom in Bob's group, the members of the lab, including the girls, sat around a conference table. Bob always prided himself on the fact that the work of the lab was guided and evaluated by the group as a whole, and he was fond of repeating that it was not a waste of time to include secretaries in such meetings. Often, what started out as a boring recital of fundamental assumptions to a naïve listener, uncovered new ways of regarding these assumptions that would not have occurred to the researcher who had long ago accepted them as a necessary basis for his work.

These group meetings also served Bob in another sense. He admitted to himself that he would have felt far less secure if he had had to direct the work out of his own mind, so to speak. With the group meeting as the principle of leadership, it was always possible to justify the exploration of blind alleys because of the general educative effect on the team. Fester was there; Lucy and Martha were there; Link was sitting next to Fester, their conversation concerning Link's mathematical study apparently continuing from yesterday. The other members, Bob Davenport, George Thurlow, and Arthur Oliver, were waiting quietly.

Knowlton, for reasons that he didn't quite understand, proposed for discussion this morning a problem that all of them had spent a great deal of time on previously, with the conclusion that a solution was impossible, that there was no feasible way of treating it in an experimental fashion. When Knowlton proposed the problem, Davenport remarked that there was hardly any use of going over it again, that he was satisfied that there was no way of approaching the problem with the equipment and the physical capacities of the lab.

This statement had the effect of a shot of adrenalin on Fester. He said he would like to know what the problem was in detail, and walking to the blackboard, began setting down the "factors" as various members of the group began discussing the problem and simultaneously listing the reasons why it had been abandoned.

Very early in the description of the problem it was evident that Fester was going to disagree about the impossibility of attacking it. The group realized this, and finally the descriptive materials and their recounting of the reasoning that had led to its abandonment dwindled away. Fester began his statement which, as it proceeded, might well have been prepared the previous night, although Knowlton knew this was impossible. He couldn't help being impressed with the organized and logical way that Fester was presenting ideas that must have occurred to him only a few minutes before.

Fester had some things to say, however, which left Knowlton with a mixture of annoyance, irritation, and, at the same time, a rather smug feeling of superiority over Fester in at least one area. Fester was of the opinion that the way that the problem had been analyzed was really typical of group thinking and, with an air of sophistication which made it difficult for a listener to dissent, he proceeded to comment on the American emphasis on team ideas, satirically describing the ways in which they led to a "high level of mediocrity."

During this time, Knowlton observed that Link stared studiously at the floor, and he was very conscious of George Thurlow's and Bob Davenport's glances toward him at several points of Fester's little speech. Inwardly, Knowlton couldn't help feeling that this was one point at least in which Fester was off on the wrong foot. The whole lab, following Jerry's lead, talked if not practiced the theory of small research teams as the basic organization for effective research. Fester insisted that the problem could be approached and that he would like to study it for a while himself.

Knowlton ended the morning session by remarking that the meetings would continue and that the very fact that a supposedly insoluble experimental problem was now going to get another chance was another indication of the value of such meetings. Fester immediately remarked that he was not at all adverse to meetings for the purpose of informing the group of the progress of its members—that the point he wanted to make was that creative advances were seldom accomplished in such meetings, that they were made by the individual "living with" the problem closely and continuously, developing a sort of personal relationship to it.

Knowlton went on to say to Fester that he was very glad that Fester had raised these points and that he was sure the group would profit by re-examining the basis on which they had been operating. Knowlton agreed that individual effort was probably the basis for making the major advances, but that he considered the group meetings useful primarily because of the effect they had on keeping the group together and on

helping the weaker members of the group keep up with the ones who were able to advance more easily and quickly in the analysis of problems.

It was clear as days went by and meetings continued as they did, that Fester came to enjoy them because of the pattern which the meetings assumed. It became typical for Fester to hold forth, and it was unquestionably clear that he was more brilliant, better prepared on the various subjects which were germane to the problems being studied, and that he was more capable of going ahead than anyone there. Knowlton grew increasingly disturbed as he realized that his leadership of the group had been, in fact, taken over.

Whenever the subject of Fester was mentioned, in occasional meetings with Dr. Jerrold, Knowlton would comment only on the ability and obvious capacity for work that Fester had. Somehow he never felt that he could mention his own discomforts, not only because they revealed a weakness on his own part, but also because it was quite clear that Jerrold himself was considerably impressed with Fester's work and with the contacts he had with him outside the photon laboratory.

Knowlton now began to feel that perhaps the intellectual advantages that Fester had brought to the group did not quite compensate for what he felt were evidences of a breakdown in the cooperative spirit which he had seen in the group before Fester's coming. More and more of the morning meetings were skipped. Fester's opinion concerning the abilities of others of the group, with the exception of Link, was obviously low. At times, during morning meetings or in smaller discussions, he had been on the point of rudeness, refusing to pursue an argument when he claimed it was based on the other person's ignorance of the facts involved. His impatience of others led him to also make similar remarks to Dr. Jerrold. Knowlton inferred this from a conversation with Jerrold in which Jerrold asked whether Davenport and Oliver were going to be continued on; and his failure to mention Link, the mathematician, led Knowlton to feel that this was the result of private conversations between Fester and Jerrold.

It was not difficult for Knowlton to make a quite convincing case on whether the brilliance of Fester was sufficient recompense for the beginning of this breaking up of the group. He took the opportunity to speak privately with Davenport and with Oliver, and it was quite clear that both of them were uncomfortable because of Fester. Knowlton didn't press the discussion beyond the point of hearing them in one way or another say that they did feel awkward and that it was sometimes difficult for them to understand the arguments he advanced, but often embarrassing to ask him to fill in the background on which his arguments were based. Knowlton did not interview Link in this manner.

About six months after Fester's coming into the photon lab, a meeting was scheduled in which the sponsors of the research were coming in to get some idea of the work and its progress. It was customary at these meetings for project heads to present the research being conducted in

their groups. The members of each group were invited to other meetings which were held later in the day and open to all, but the special meetings were usually made up only of project heads, the head of the laboratory, and the sponsors.

As the time for the special meeting approached, it seemed to Knowlton that he must avoid the presentation at all cost. His reasons for this were that he could not trust himself to present the ideas and work that Fester had advanced, because of his apprehension as to whether he could present them in sufficient detail and answer such questions about them as might be asked. On the other hand, he did not feel he could ignore these newer lines of work and present only the material which he had done or had been started before Fester's arrival. He felt also that it would not be beyond Fester at all, in his blunt and undiplomatic way (if he were present at the meeting, that is) to make comments on his own presentation and reveal the inadequacy which Knowlton felt he had. It also seemed quite clear that it would not be easy to keep Fester from attending the meeting, even though he was not on the administrative level which was invited.

Knowlton found an opportunity to speak to Jerrold and raised the question. He remarked to Jerrold that, with the meetings coming up and with the interest in the work and with the contributions that Fester had been making, he would probably like to come to these meetings, but there was a question of the feelings of the others in the group if Fester alone were invited. Jerrold passed this over very lightly by saying that he didn't think the group would fail to understand Fester's rather different position and that he thought that Fester by all means should be invited. Knowlton then immediately said that he had thought so too and that he felt that Fester should present the work because much of it was work that he had done; and that, as Knowlton put it, this would be a nice way to recognize Fester's contributions and to reward him since he was eager to be recognized as a productive member of the lab. Jerrold agreed and so the matter was decided.

Fester's presentation was very successful and in some ways dominated the meeting. He attracted the interest and attention of many of those who had come, and a long discussion followed his presentation. Later in the evening, with the entire laboratory staff present, in the cocktail period before the dinner, a little circle of people formed about Fester. One of them was Jerrold himself, and a lively discussion took place concerning the application of Fester's theory. All of this disturbed Knowlton and his reaction and behavior was characteristic. He joined the circle, praised Fester to Jerrold and to others, and remarked on the brilliance of the work.

Knowlton, without consulting anyone, began at this time to take some interest in the possibility of a job elsewhere. After a few weeks he found that a new laboratory of considerable size was being organized in a nearby city, and that the kind of training he had would enable him to get a

project head job equivalent to the one he had at the lab, with slightly more money.

He immediately accepted it and notified Jerrold by a letter, which he mailed on a Friday night to Jerrold's home. The letter was quite brief and Jerrold was stunned. The letter merely said that he had found a better position; that there were personal reasons why he didn't want to appear at the lab any more; that he would be glad to come back at a later time from where he would be, some 40 miles away, to assist if there was any mixup at all in the past work; that he felt sure that Fester could, however, supply any leadership that was required for the group; and that his decision to leave so suddenly was based on some personal problems; and he hinted at problems of health in his family, his mother and father. All of this was fictitious, of course. Jerrold took it at face value but still felt that this was very strange behavior and quite unaccountable since he had always felt his relationship with Knowlton had been warm and that Knowlton was satisfied and, as a matter of fact, quite happy and productive.

Jerrold was considerably disturbed, because he had already decided to place Fester in charge of another project that was going to be set up very soon, and had been wondering how to explain this to Knowlton, in view of the obvious help and assistance and value Knowlton was getting from Fester and the high regard in which he held him. He had, as a matter of fact, considered the possibility that Knowlton could add to his staff another person with the kind of background and training that had been unique in Fester and had proved so valuable.

Jerrold did not make any attempt to meet Knowlton. In a way, he felt aggrieved about the whole thing. Fester, too, was surprised at the suddenness of Knowlton's departure and when Jerrold, in talking to him, asked him whether he had reasons to prefer to stay with the photon group instead of the project for the Air Force which was being organized, he chose the Air Force project and went on to that job the following week. The photon lab was hard hit. The leadership of the lab was given to Link with the understanding that this would be temporary until someone could come in to take over.

Chapter 18

UNITED DIESEL CORPORATION*

UNITED DIESEL CORPORATION was a large-scale manufacturer of diesel engines for locomotives and other heavy equipment. The market was a highly competitive one, and virtually every engine was uniquely constructed to customer specifications. In recent years the top management of United Diesel had become concerned about the apparent decrease in creativity of design and the ever increasing time required to develop designs and drawings for the manufacturing department.

A researcher in organizational behavior became interested in this problem and was invited by management to investigate its causes. His first days were spent visiting with various members of the design engineering staff and the drafting group.

As the researcher busied himself studying company background material at a desk in the engineering office, he overheard two engineers talking.

RALPH BURKE: Jim, one guy you want to steer clear of is Parker.[1] You know what he just did to me? I was in a meeting on that Crafts job. The customer's not happy, so Parker got everybody together who's been in on it. He didn't like the design and, I admit, he had some good points. But, do you know, he criticized engineering's judgment right in front of the drawing office people. Why couldn't he have talked with the engineers first so we could have headed the thing off before it got to a public demonstration? But, no, he had to pull out our dirty linen in front of a lot of drafting people who expect us to know what we're doing. Parker's supposed to be interested in simplifying communication between the customer and the draftsman, but all he's doing is making the drawing office lose faith in our judgment. It was all I could do to sit there while he questioned my design ability, with those drafting people sitting back and taking it all in.

PACKARD: Yeah, I know how you feel. We spend a hell of a lot of time figuring out a job in detail before passing it on to the drafting boys to make sure they feel they can trust our judgment that everything is going to fit together and work properly. Then, some chump like Parker puts his

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[1] Philip Parker was a sales executive in the company.

foot in it, and the draftsmen don't know what's up. You and I both know how important it is to a draftsman to know just what's expected of him and how he's supposed to develop a design. This is what they need to know and what they want to know. But, without some faith in our ability to give them the right information, they won't know where to turn. Someone ought to set Parker straight."

The researcher also had a number of opportunities to talk with draftsmen about their jobs and their working conditions, and especially about their relationships with the design engineers.

One oldtime design-draftsman told the researcher:

All the motivation to do good, creative designing has been lost around here. Take a chemist, or a teacher. He doesn't care about money all the time; it's his work that he's interested in. It's what he can do. It used to be that way before, but not any more. Nowadays, nobody has any interest in what they're doing. They just tell you to draw some lines, and you draw them. In the old days, you would work on it with the engineer, you'd make your own layout, you would even do your own detailing, and you would work with the guys in the shop. Everything. In those days you had responsibility for a whole job, for a whole engine, and you were interested in what you were doing, and you knew your work, and you were good at it. I could design a whole engine in the old days. There ain't a guy here in the whole department that could do that now.

I'm not surprised that nobody's got any incentive or wants any responsibility. In the old days, I would be so wrapped up in the job that I'd run out to the woods at night, clench my hands till they bled and scream with frustration because I couldn't figure something out on a design. Hell, nowadays, it's all I can do to stay awake. I'm not interested any more. I could go to work any place. I'm a trained draftsman, not a line drawer. I'm a layout man. I could work in automatic machinery, electronics, or any place.

A little later, the researcher talked to Henry Nelson, a senior design draftsman. He said:

I entered the employ of Consolidated Diesel in 1923 as a draftsman with a background of six years of drafting experience with several concerns. Why had I chosen drafting as a career? As a technical school student, I worked two summers in a drafting room and became quite impressed with the prestige which the draftsmen (draughtsmen in those days) enjoyed, so the die was cast.

Soon after starting with United, I had the opportunity to do more and more work of an engineering nature, so I did considerable home study to qualify for the transfer to engineering. It finally came at the end of six years and lasted about three years until the depression came, along with a new department manager. Because I had no college degree, I was transferred back to drafting.

This brings up the point that United has a barrier which cannot be passed by a draftsman unless he possesses a degree, whereas in the automobile plants and many other plants, a man can go as far as his abilities can take him.

Another point is the fact that if a man is exceptionally good at a given job, he finds himself pegged and often bypassed from promotion in favor of someone who may be less capable in the job he is holding. This means we have in some cases eunuchs who tell you what to do, even though they cannot do the job themselves.

The draftsman in United no longer enjoys the prestige of years gone past but rather is considered a necessary evil or burden. While it is true that the draftsman is to a certain degree dependent on and guided by the interested engineer, he nevertheless interprets and executes the necessary working drawings for production of the design.

. . . A few years ago I asked for a wage increase, and after six months or more I was told that my name was on the list. In an interview with my supervisor, I was told that quite often I would design to suit myself rather than follow the engineer's wish. My reply was that I first followed the engineer's instructions, and then to avoid sitting around thumb-twiddling, waiting for the engineer, I would try to work up other possible solutions. About six months later, I did get my increase, which actually was two years overdue.

The ironical part is that about one year later "Value Analysis Seminars" were held for two large groups in two sessions of about five weeks' duration. One of the points stressed was that there was probably a better way of doing the job; consequently, all ideas should be studied, not immediately rejected, as is too often the case when it differed "from the way it has always been done for years."

A younger draftsman explained:

A good draftsman is one who will take responsibility. Most of the men around here would do it if they only had more information. As it is now, nobody knows what's expected, so all he does is just exactly what is required.

Another designer at the next board heard this last comment and chimed in:

They always say the engineer doesn't know what we're doing anyway, but he's got to act as though he does. So, when you show him a drawing, he'll say, "Change this radius here," when the radius doesn't have any significance at all. They should respect our feeling for a problem and not always give an answer when they come in cold on something we know a lot about. Of course, they see a lot out in the field that leads them to infer that there are problems, but they shouldn't feel they always have to have an answer.

A middle-aged designer joined the group. He said:

Yeah, engineers always want to make changes in your designs. I don't have any trouble with Richardson. He always approves my work, but Burke has to play the game. He didn't use to check my work. Then he made a stink, and now he does. He has to go over and over it and make suggestions, but nine times out of ten he comes back to my original design."

And an older designer added:

The tendency of an engineer is to get unhappy when a designer puts his own ideas in and goes beyond the engineers. I've never felt I had anything extra to contribute from a design point of view. I give them what they want and express my own thoughts and ideas when I've got them, but not very forcefully. Mostly the design ideas are a matter of detail (like wall thickness or contour or something). It's never a very big thing because we always work on about the same kind of designs. So, what I have to offer is always going to be equal to whatever the engineer wants."

* * * *

Several days later, the researcher posted himself in the drawing office, his eye open for contacts between draftsmen and engineers. At 8:30 A.M., Henry Nelson, whose comments were quoted above, invited the researcher to join him in a visit with Ralph Burke in the engineering office. As the two men walked the few hundred feet between offices, Nelson described the reason for his call on Burke:

NELSON: Ralph came down to my drawing board a few days ago in a rush to get going on the packaging design for the Proxmire job. I had already done a crude layout of the package over a month ago, but it had to be put aside because we didn't have the final specs. I was just finishing another project when Ralph told me he had the Proxmire data, finally, and was most anxious to get underway. So I've been pushing hard to complete the job I had on my board, and now I'm free to go ahead with Burke.

It turned out that Burke was temporarily out of the office, so Henry left him a note. Back at his drawing board, Henry made further sketches, outlining his ideas for the Proxmire package. Whenever someone walked by his work place, he glanced up, as if expecting Burke. On several occasions Burke came into the drawing office to consult with other draftsmen, but he did not approach Nelson. Henry remarked to the researcher that he could not understand the delay, in view of Burke's previous insistence on getting the job started.

At 4:00 P.M., a clerk informed Henry that Burke was ready to see him. Henry gathered his sketches and, again accompanied by the researcher, walked to Burke's office. On Ralph's desk was a copy of the preliminary package layout which Henry had drawn. It became the focus of their conversation.

BURKE: Henry, you won't have to build the package as high as it's drawn here. The latest information indicated that a shorter package will allow sufficient maintenance access.

NELSON: All right. Do you know yet just how the package will connect with the customer's installation for it?

BURKE: Specific details on their installation aren't necessary for this job. If you keep the height to a minimum, there should be no problem of fitting the engine into their machine. Now you'll want to use a corner radius of 4 inches on this size job."

NELSON: Four inches—just the radius for all the other jobs of this type.

BURKE: "That's right. Just follow the ideas on previous layouts.

NELSON: What ideas did the customer give you about designing the package so it can be taken apart whenever repairs to the engine have to be made?

BURKE: [*After several minutes' silent study of the drawings.*] I don't think any changes over what we knew before have been made in their specs on that aspect of the job.

NELSON: Well, we could handle the disassembly in a number of different ways and, as I recall, no specific choice had been made up to now.

Burke sat back and again silently studied the drawing. After several minutes Nelson assumed that Burke was not going to respond to his question. Henry pulled the sketch sheets he had been working on out of his pocket and spread them on top of the layout drawing.

NELSON: Here are some sketches I've been working on which seem to be flexible enough to take care of all possible repair contingencies, but the design wouldn't be so complex as to be uneconomical, I don't think. You see, the panel on this end could be broken out to allow for this section of the engine to be removed without disassembling the whole package.

BURKE: [*With a brief glance at Nelson's sketches.*] The chances are that if anything breaks down, it will be this section over here. That would require taking the whole package apart anyway. Your end panel idea would probably be used so seldom that it wouldn't be worth including. No, just design the package the way you've done similar ones.

NELSON: Well, if that's all then, I'll get started on the final layout. What drawings should I look at to base this design on?

BURKE: It would be the XYZ job, but you'll have to look up the drawings. I wouldn't know which ones would be relevant. And you might as well take this layout drawing with you. I have no use for it.

NELSON: I'll get started on this as soon as I can.

* * * *

After Nelson's meeting with Burke, the researcher asked Henry what his reaction to it had been.

NELSON: Well, I was a little surprised that Ralph knew so little more than he had known before. I guess he's trying to keep things open so he can change his mind later. A lot of the engineers seem to work that way, particularly the young ones. They put up a front, as though they didn't have anything left to learn. They're afraid we'll find out how green they are. It hurts their pride to have a mere draftsman giving them answers or ideas. Well, I guess I can junk these sketches and do it his way. I won't bother him again with my ideas. My mistake was thinking that he wanted some help in the first place. For a guy who's been around here a long time, I can be pretty dumb sometimes.

Not long after the meeting between Nelson and Burke, the researcher found an opportunity to ask Ralph Burke what qualities in a draftsman he most valued.

BURKE: A draftsman should have good visual imagination and be able to create things. He should be able to give you what you want the first time you ask for it. Of course, that means we have to explain what we want and how we want it done, clearly and in detail. The trouble is that too many draftsmen spend valuable time putting extraneous ideas into the design.

You have to keep a tight check on their work to make sure they haven't fouled up the machine. It's okay if they do something on their own *after* they've done what we've asked them to do.

Take Henry Nelson, for example. You were there the other day when we went over the Proxmire job. That's an important order, and we're way behind schedule on it. Henry knew it had to be completed quickly. And yet, he wanted to play around with some fancy ideas that weren't necessary and might well have weakened the overall design. If he had spent the time he put into those sketches digging out background material from other jobs to build the basic design on, he'd be living up to my idea of how a good draftsman should behave."

* * * *

A short time later the researcher talked with the director of engineering of United Diesel, Mr. Buckley. Buckley was responsible for all design and preproduction activities throughout the entire company.

BUCKLEY: I don't really understand it. Men just don't seem to want to work hard anymore. Why, I can remember when we were smaller and I was a design engineer, we'd work 'til two or three o'clock in the morning, night after night, when we were trying to solve a difficult design problem. It was an enormous challenge to try to solve problems which we had no idea how to deal with. All of us had to learn a tremendous amount, and we thirsted for the knowledge and experience that would help us.

But today all the men seem to be interested in is the money they make and their leisure time to enjoy it. Sure they want job security too—but the challenge of the job itself is done. That's particularly true of the draftsmen. You know, those fellows like to think of themselves as sort of "engineers," and they get very sensitive about their status and prerogatives. But then they turn around and just barely work hard enough to get by. You have to keep after them all the time. Now that's no way for a "professional" man to act. If they want recognition, why don't they earn it by hard, creative work?

Chapter 19

ACME AIRCRAFT CORPORATION*

GEORGE BRUSTER took an engineering job with the Acme Aircraft Corporation soon after his graduation from State Engineering College in June of 1959. He was initially assigned the responsibility of supervising a group of engineers and engineering aides involved in conducting experimental test programs on various models of airplanes and airplane components.

Prior to his graduation, Bruster had spent several summers working for Acme Aircraft and had worked part time for this organization for a period while attending school. At the time of his permanent employment, he had held every position in the testing organization other than that of crew chief. Because of his previous experience, he was assigned the position of crew chief—an unusual assignment for a "new" engineer.

The average size of a testing crew was seven to ten individuals, of whom five were usually graduate engineers and the remainder engineering aides and technical assistants. The responsibilities of the crew chief included the overall planning and coordinating of the test programs with which his group was involved. Approximately half of the crew chief's responsibilities were administrative rather than technical; and because of the nature of his responsibilities, the crew chief often was not the engineer on the crew who had had the most experience or the greatest technical knowledge. Often older engineers, specialists in electronics or design, for example, would be working on crews headed by younger, less-experienced engineers. This type of arrangement had rarely created friction in the past—especially as the older specialists were usually not interested in accepting responsibility for anything but their particular part of a testing series. In addition, crew membership was constantly changing as crews were disbanded and reformed as tests were completed and new tests undertaken.

Exhibit 1 is an example of the organization of a typical testing unit.

On November 1, 1959, George Bruster's group was assigned the project of testing a new model component for an experimental aircraft which Acme was developing. It was only the second major assignment that George's group had had since he took over as crew chief in June. This

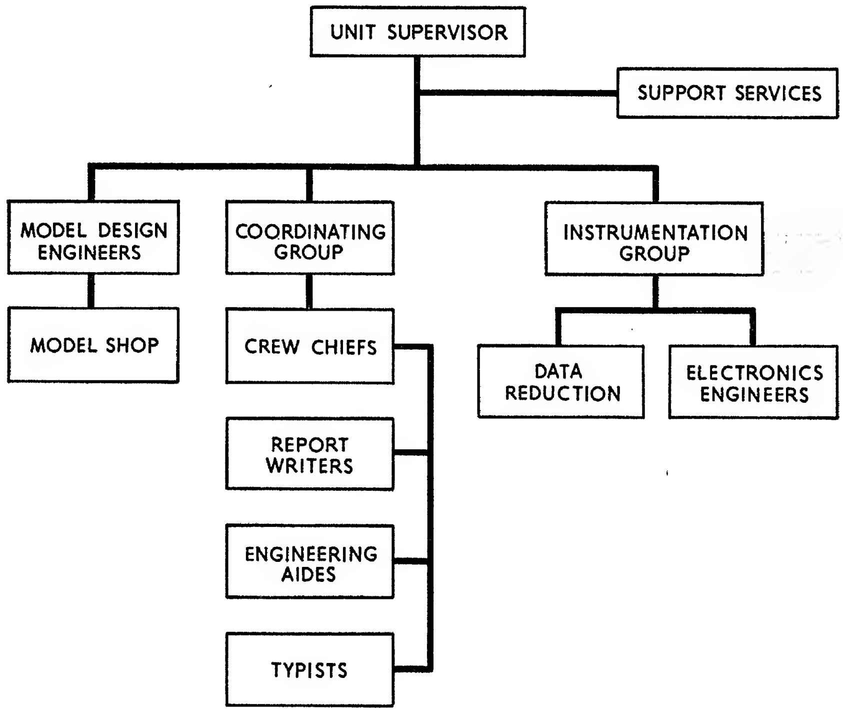
* From *Human Elements of Administration,* by Harry R. Knudson, Jr., copyright © 1963 by Holt, Rinehart and Winston, Inc. By permission of the author and the publishers.

particular test series was of prime importance, for the results of the tests would be instrumental in providing data upon which Acme would base its design proposal to government representatives. If the company could provide an acceptable design, it would be in a favorable position to eventually gain a large—and profitable—production contract.

The unit supervisor stressed the importance of this particular series of tests to George and informed him that the entire testing group was under

Exhibit 1

ORGANIZATIONAL CHART OF TESTING GROUP



considerable pressure from top management to get quick and accurate results. Top men from the model design and instrumentation groups had been assigned to the tests, as well as one of the best report writers in the coordinating group.

Because of scheduling problems regarding the test facilities available at the Bristol plant of Acme—the "home" office of the test group headed by Bruster—arrangements were made for the current series of tests to be conducted at the Culver City facilities of Acme. Culver City was located approximately 900 miles from Bristol; and while the Culver City operations were organized in the same manner as those at Bristol, the operations were run autonomously because of the physical distance between the two facilities. Thus, while Bruster and his crew would actually conduct the

tests, they would be using the equipment and facilities of the Culver City operation.

George Bruster arrived at the Culver City test facility on November 7 to coordinate with the people there and make final test arrangements. After determining that the test was scheduled for December 1 and that his crew would receive the full cooperation of the local facility, he went to the data reduction group to arrange for the IBM processing of data that would result from the test. The head of the data reduction unit, Gil Harmon, introduced George to the chief programmer, Dick Jones, with whom he was to work throughout the data reduction process.

George explained the importance of the test and showed Dick the type of information that would be required as final data from the digital computers.

After studying the information for a few minutes, Dick said, "I'll have to write a new program in order to give you the information you want. None of our present standard programs are capable of handling this job."

"How long will it take to write a new program?" George asked.

"Oh, I could probably have it finished by the fifth of December. That's the last day of your test, so it will be done in time to reduce your data."

"That won't do!" George exclaimed. "We must have final data from day to day all during the test. This proposal is red hot, and we must analyze our final data on a day-by-day basis. That's the only way we can be sure we are taking the right approach in our test program. Each day's testing will be dependent on the final data from the day before."

"So, what do you think? Can you have your program finished by the first?"

"Well, I dunno," mumbled Dick. "I might be able to finish by the first if there are no hitches in the program, but things seldom go that smoothly."

"But it is possible to finish by the first?" insisted George.

"It's possible, but I have other important programs to work on. Everything would have to work properly the first time."

"This project is so important it just has to be done on time. We don't have any choice but to plan it for the first of the month."

"Okay, I'll give it all the effort I can. With a little luck it will probably be ready for the first."

"Swell," concluded George. "I'll count on it."

After concluding all pretest arrangements, George returned to Bristol. During the remainder of November he was in frequent telephone communication with Culver City. All preparations were progressing satisfactorily, and Dick Jones assured George that the program would be finished by the last day of November.

On the 30th of November, George and his crew arrived at Culver City

to begin the installation of their model. The test was to begin the next day.

George found everything in readiness for his test, except that the data program was not quite finished. He went to Dick Jones and asked what the holdup was.

"No holdup," Dick replied. "The program will be completed by the end of the day, and we can check it out on the computer first thing in the morning. If everything checks out okay, we will be able to run your data from the first day's testing sometime tomorrow night. So you'll have your final data the next day, just as you requested."

"What happens if everything doesn't check out?" asked George.

"It'll take a little while to iron out any bugs that may show up. But it shouldn't hold us up much; a few hours maybe."

"Will you be able to work overtime on this if it becomes necessary?"

"I think so. We should be able to get your data for you one way or another, so don't worry. We'll let you know if we run into any major problems."

George was reassured and returned to his hotel satisfied that all was in readiness for the start of his test the following day.

The testing groups existed as staff units. As such, they conducted tests at the request of line and project groups who were in need of the particular information. It was the usual practice for the group requesting a test to send along a representative to make whatever decisions regarding the test program that might come under the jurisdiction of the line organization.

The requesting group for this test had sent along their senior project engineer, Richard Wallen, because of the importance of the test. Wallen was a fairly new supervisor but was well qualified technically. He was a "driver," worked his subordinates hard during rush programs such as this, and had a reputation of sometimes "rubbing people the wrong way" in order to achieve an immediate goal.

Wallen was directly responsible to the division general manager for the success of the program. One of his major concerns was whether or not the final data would be ready on a day-to-day basis. He asked George about it the night before the test.

George replied, "Dick Jones told me everything would be ready on time. The odds are real slim that anything would go wrong; and if something went wrong, it would only slow us down a couple of hours."

The following day the data from the first shift of testing was turned over to Jones for processing. However, the program did not check out properly, and Jones was unable to give George his final data the following day.

The same situation occurred the next two days of testing, with Jones unable to make his program work despite working several hours overtime each day.

On the fourth day, the results were no better. Three hours after the testing shift was completed, Wallen and Bruster went to see Jones. When they found that Jones had gone home, Wallen "blew his top" and told George Bruster to telephone Jones at his home and demand to know why he wasn't at the office working on the programming problem.

A few minutes later Gil Harmon received a worried phone call from Jones, who quoted George as saying, "If you don't get down here and start working on this program, it may cost you your job!"

Harmon passed on this information to Conners Simpson who was instrumentation supervisor at Culver City. Simpson was shocked and angered at the attitude the visiting group was taking toward one of his men. He immediately phoned Wallen and demanded an explanation. He told him in no uncertain terms to "lay off" his men and also told Wallen to follow the proper chain of command and notify him first next time there was a problem. In addition, Simpson stated he "had no intention of letting Jones come down and work on your damn program. Jones has been working 12 hours a day for the past week and has several other problems to deal with also. Besides, it's too late to salvage much of the data."

Before he hung up the phone, Simpson told Wallen, "This thing has gone too far. I'm going to take it to the boss and get it ironed out first thing in the morning. I want you and your crew chief to meet me, Jones, and Harmon in the boss' office at eight o'clock tomorrow morning."

The meeting was held and the difficulties were straightened out, but the test series was considered unsatisfactory by all involved. The group had failed to get the desired information.

The meeting proved to be a unique experience for George Bruster. Wallen denied outright that he had told George to use the strong language that had upset everyone so badly in speaking to Jones.

George could only reply that he thought that such language was Wallen's intent. George subsequently left the meeting shaking his head in an unhappy manner and trying to understand how the whole affair had deteriorated into such a mess.

Chapter 20

DALLAS CHEMICAL CORPORATION*

IN THE SPRING of 1959, Dr. Caldwell,[1] the director of research at Dallas Chemical Corporation, called a meeting of laboratory management to discuss the problem of formal recognition of scientists who stayed in research work. Attending the meeting were the personnel manager, Mr. Keats, and the two laboratory directors, Dr. Faulkner and Dr. Maughan. Six months before Dallas' research division had initiated a "technical ladder of advancement" for those scientists who chose to remain in direct scientific work rather than assume research management positions. The purpose of this meeting was to analyze the effects of this technical ladder and arrive at some decisions on future action in this area.

The research laboratories of the Dallas Chemical Corporation were a separate entity and maintained only loose ties with any of the other corporate divisions. They had been located in a small university town for the past 15 years, and much of the academic atmosphere pervaded the laboratories. No development work or engineering was done by the professional staff of 400 persons; they were strictly basic researchers in all areas of Dallas' interests.

Dr. Caldwell opened the meeting by reviewing the events leading up to the company's initiation of the technical ladder of advancement.

DR. CALDWELL: Let me briefly review the events which led up to our decision last year to initiate a technical ladder. Our interest, as you may recall, was stimulated about a year and a half ago by both internal and external forces. You may recall the loss of several highly regarded individuals to other companies for reasons that appeared to be better positions and increased status. At the same time there appeared in the literature several articles pertaining to various methods of rewarding technical personnel. At that time, Mr. Keats made a study of the procedures and policies used by several other laboratories comparable in size and work scope, and he presented a plan to our research council for the adoption of the title of Fellow of the Technical Staff. This was to be the only step in the ladder and was to be the highest status our technical men could obtain. Six months ago we made a public announcement of this

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[1] All names disguised.

position plus the appointment of eight of our best scientists as "Fellows." These men are, as you are well aware, outstanding in their fields and also our most senior men. Now that six months have passed I would like to hear from you any reactions of your own or your men concerning the position plus your recommendations for the next step to be taken.

MR. KEATS: Since I made the original study at the time we adopted the Fellow title, I have been doing a lot of thinking and talking with outsiders on this action. When I first presented our plans to the research council, I rationalized this move on the basis of having a desire to show to everyone associated with these labs that management wants to reward those men who have contributed to the company's benefit over the years. The other reason was a desire to remove the lack-of-a-title stimulus which drives some of our good men out of research. Let me explain this last one in more detail. For years we have done our best to give the researchers whatever they wanted within reason. One very selfish reason we have done this was because the researcher who saw a difference between his present job and a job in management or a research job at another company would be much more prone to leave for this reason. One of the areas in which these differences occur is the title and status of the job. Before this dual ladder was conceived, this was one area in which research was particularly weak. Ours is a management-oriented society where a man's position depends upon his title. When I say this was our weak area, I mean we were losing men to other labs and to nonresearch work when everything but the title was the same. Thus our introduction of the technical ladder was our first attempt at correcting the title problem.

DR. FAULKNER: Six months ago, when this idea was originally presented, I agreed with you; but from reactions to the dual ladder in my lab, I conclude that this is not practical at Dallas Research. Let me show you where these arguments fail when applied to our research staff. In my lab, recognition of men is primarily through the work they are doing and the articles they have published. Management cannot confer prestige and recognition; it can only hope to recognize formally this "position" of these men who have done well in their profession. In fact, our first action of conferring the title of Fellow on those eight outstanding men was primarily for the purpose of borrowing some of their prestige for the title. This earned recognition which I am speaking of pertains only to the laboratory and the scientists as a professional group. Certainly this title has an effect, but upon whom? Obviously, upon the people who cannot recognize a man for what he has done but only for the title he wears. This comprises most of the rest of the corporation plus the outside world, the man's family, and his friends. Perhaps this is the group we want to please, but I think not. What does it matter what they think of you? This is hollow prestige praise for a title, not for the man or the accomplishments. As far as removing the title difference which might motivate good researchers out of science, I doubt that this is important; the good

researcher has such a strong motivation to contribute that the quest for a title is a comparatively weak force.

DR. MAUGHAN: Without appearing to be a fool I would like to agree with both you and Keats. I think you are both correct but that you are talking about different people. Perhaps we have diagnosed the problem correctly but treated the wrong patient. By that I mean that the eight men who have received the titles were the last ones to need them. They had recognition, money, honors, and whatever else was needed. The Fellow title was only frosting. However, there *is* a group which is drastically affected by management's attitude toward titles and that is the younger men; these are the ones who have not yet received formal recognition and are most prone to leave the lab because of their poor status. At this point I want to disagree with Dr. Faulkner on the importance of being recognized outside the professional group. Many of these men have families and friends who are important and are interested in how they are doing, and to these people "how they are doing" is equated with their title. The older men, their wives and families, have learned to live with their position, but to the young men it is doubly hard, for they see their friends in business and the universities climbing the various ladders and they have no signs of progress to show. As a generalization, I also think that the younger generation is more title-conscious. To state my position fully, I think that the need for a title provides a motivational force of varying intensity for the individual, but on the whole stronger in our younger and less well-recognized colleagues. I think that the pressure from the outside can be strong enough in some cases to veer a good researcher from strictly scientific work in our labs to other labs or nonscientific work. As an aside I think that the size of the lab has grown to the point where a man may no longer be recognized for his accomplishments alone as all of our Fellows were. It now may take the formal approval of the lab management before a man is recognized for his work, even within the lab.

MR. KEATS: If we accept your hypothesis of the value of the title being dependent upon the position of the man receiving it, this would tie in nicely with the results which I have observed. For one thing the results have been very negligible; no one is either very pleased or displeased. Most of the staff are waiting to see if any other titles are introduced and who will be recognized in the next few years. By giving more men recognition and recognizing the younger men, management will make a real contribution to their positions both within and outside the labs.

DR. FAULKNER: Yes, and it is these younger men who may be hurt the most by this system of giving titles. We haven't mentioned any of the ill effects of this "ladder." My men have commented that one of the big advantages of our lab is its democracy, and they are worried that formal titles would result in a stratification detrimental to communication. One or two of them left labs just because of this; for example they don't like to see all the senior scientists eating together and loss of first-name intimacy.

Many also fear that the granting of titles will produce a harmful competition for titles which could endanger the cooperation and communication between our scientists. One of our big selling points in recruiting men has been this lack of competition which created a different atmosphere than at many labs. The final thing that I think we should consider is what the effect of not receiving a title is. How will it affect the performance of the man who doesn't get the title, both the mediocre one who doesn't rate one and the good man whom we misjudge? I realize that the first step has been taken and we can't turn back, but I certainly think we should go slow in the future, first comparing the advantages that have been mentioned against the liabilities of each new step.

Mr. Keats: I have to agree with most of your points, but the problem is what can be done about this reaction? How can we even measure it? To your list of problems with the technical ladder I would like to add one more. A major complaint is that we give the men a title only and no other rewards such as a bonus, a raise, or many of the little trappings they expect with high management positions. It is difficult to tell the younger men that the Fellows have been on the same financial level as members of the laboratory management for years. I have tried to point out that it is our policy to pay a man equally for his contribution, whether he is in a research management position or actual scientific work. Most of the younger men don't believe this, and it is impossible to prove this with our policy of salary secrecy. As far as the trappings of the position go, you all recognize that our policy has been to minimize the trappings in the entire laboratory. We have no private parking lots or dining rooms, and researchers obtain space, service personnel and equipment only when they can show a need. This puts us in a definite bind as compared to other labs or even some product divisions of our own company in which salary increases and all sorts of little trappings are included in a promotion or new title.

Dr. Caldwell: As I have been sitting here listening to the arguments both pro and con for this technical ladder it strikes me that we are involved in a problem much more complex than originally anticipated. If we accept the hypothesis of Dr. Maughan that the real effects of this title business are felt most strongly in the middle echelons, then we can concede that our first efforts have been negligible because they were misdirected. These top men had all the recognition they needed; the Fellow title was frosting on the cake. However, these first efforts were not entirely wasted, for two reasons: first, we had a definite desire and duty to recognize these men for their contribution to Dallas' research; second, this move has acquainted the entire technical staff with the knowledge that management wants to recognize them formally and their time will come. As I see it, we are now progressing to more reactive material—those men who have not been widely recognized. Selection of these men will be much more controversial. The reactions here to our actions should be

much more substantial, and we need to give careful thought and planning to the situation to make sure that the reaction is favorable. As to the problem of not providing any physical rewards with the title, I think much of this may be solved by my explaining our salary policy of not associating a raise with a promotion, but publicizing the fact that the top salary barriers have been removed for this group of men.

Specifically what I would like to see come out of this meeting is a decision as to what our next step should be, naming more Fellows or initiating a new position for younger men or both. This entire problem has suddenly been exposed as an iceberg with the most dangerous part hidden. I do not think it appropriate or possible to arrive at a decision immediately. Let's meet again next week at this time. That will give you some time to sound out the men in your groups. I hope by next week we can detail a course of action on this subject.

Chapter 21

FORT WORTH PHARMACEUTICAL AND CHEMICAL CORPORATION*

IN THE SPRING of 1959, Wayne Malvern[1] was spending many hours a day worrying about the difficulties involved in adding polyethylene to the list of profitable products manufactured by the Fort Worth Pharmaceutical and Chemical Corporation. Wayne knew that profitability depended upon the ability of all concerned to deal with serious technical problems which had emerged from the polyethylene process and was very much aware of his personal responsibility for initiating ideas and coordinating the efforts of those who might contribute to the solution of these problems.

Wayne was a staff member of the development division of Fort Worth's central research laboratories. The function of this division had been formally stated in a brochure circulated throughout the company as "first, to do the staff work needed by management to guide the company's chemical research and, second, to handle the process engineering and economic evaluation of chemical projects during their development stages." Or, as stated by Dr. Wynn Cote, one of the senior engineers on the development division staff, "We are responsible for a process that makes a product of specified quality."

Fort Worth had been a major producer of pharmaceuticals in the United States for over 40 years. After World War II, the company began to diversify into other phases of the chemical industry. Annual sales were about $750 million in 1958, with chemicals contributing a steadily growing percentage.

New product research and development and engineering of new plant facilities had been centralized in the central research laboratories since before World War II. Both pharmaceutical and chemical R & D were done at this laboratory, which was located in the suburbs of a large western city. As the chemical business increased, the size of the technical staff working on purely chemical R & D had increased proportionately until nearly 400 professional people were employed.

There were three major groups in the chemical R & D laboratories; (1) the chemical research division, (2) the applications research division

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[1] All names disguised.

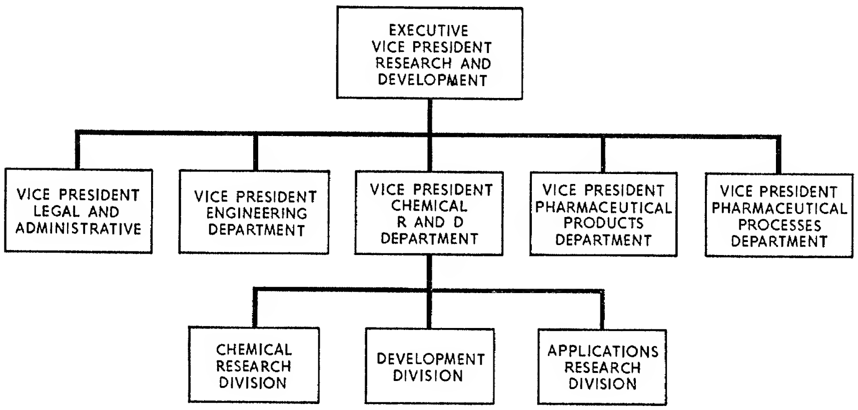
and (3) the development division. Also housed in the central laboratories was the engineering division, which planned, designed, and constructed plants for all product divisions of the parent company (see Exhibit 1). Intimately associated with the work of the chemical research and development divisions were the company's pilot plants and associated laboratories which were located many hundreds of miles from the central laboratories on the grounds of the company's major production facility.

The development division was staffed by 25 chemical engineers. These men were grouped rather informally under section heads according to

Exhibit 1

FORT WORTH PHARMACEUTICAL AND CHEMICAL CORPORATION

Partial Organization Chart
Central Research Laboratories



areas of technical interest (see Exhibit 2 for details of the simple organization of the division). Wayne Malvern was, in 1959, a member of the specialty products section. He had a B.S. and M.S. from a large eastern university and had taken all the course work for his Ph.D. in chemical Engineering before coming to work in the development division in 1955. By June of 1959, he was a staff engineer responsible for four products in polymer chemistry, of which only two were currently active. His work on one of these had been nearly completed since the commercial plant built to produce the product had commenced operations and had been evaluated as successful. His activities in connection with this product were therefore now confined to answering questions posed by people at the plant.

The fourth product, polyethylene, presented major technical problems, and it is around Wayne Malvern's attempts to deal with these problems that the present case came to the attention of Bob Handy, a researcher from the Harvard Business School.

Wayne's section head, Dr. Glen Side, had been associated with the

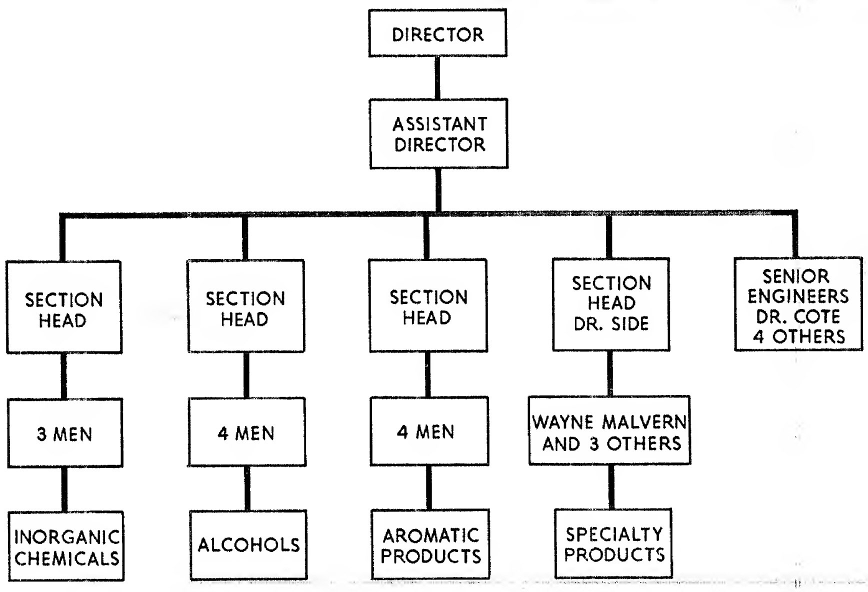
division as staff member or head of various sections for eight years. He had just recently taken over the direction of the specialty products section and felt he was "still getting his feet on the ground" as far as his understanding of the problems presented by particular projects was concerned.

In addition to section heads and the staff members assigned to particular sections, the development division was served by five senior engineers. One of these senior engineers, for example, was Dr. Wynn Cote. Dr. Cote had about 15 years' experience with the company, some in engineering design and some as a consultant to a commercial division. In common

Exhibit 2

FORT WORTH PHARMACEUTICAL AND CHEMICAL CORPORATION

ORGANIZATION OF DEVELOPMENT DIVISION



with the other four men bearing this title, he reported directly to the division directors. Describing his job, Dr. Cote said, "My job is almost the same as Wayne's with the exception that I have very little supervision over me. Only on policy questions such as decisions regarding the need for a project or the priority a project should receive do I go to my superiors." The projects Dr. Cote worked on were similar to those assigned other staff members. Formally, he was free to do any work within the spectrum of the division's activities; informally, he usually received projects closely aligned to his background and interests. It was clear to Bob Handy that the senior engineers and chemists enjoyed considerable status in the chemical research and development laboratories. They all had private offices and walnut desks comparable to those of section heads, and while salary position was a well-guarded secret in this

organization, it was an accepted fact that senior engineers' and chemists' salaries equaled and often surpassed those of section heads.

It had become increasingly clear to Handy, as he talked to members of the development division staff and observed their activities, that their major problem could be expressed as a need to get necessary experimental work done by researchers and engineers in other divisions. The development division had no authority to order this work, no money to finance it, nor any laboratory or experimental staff to do it themselves. At the same time, Handy realized that the laboratory divisions had responsibility for getting their own work done. All the development division staff had to work with were their technical skills and their ability to form good relationships with those who did have the necessary laboratory facilities and money to do the experimental work.

In explaining the polyethylene problem to Bob Handy, Wayne said "We have a commercial plant coming on line in four months. We can sell only 20 percent of the plant's estimated capacity because of a product quality defect. Originally, the plant was planned for a lower capacity, solely for internal consumption. Several large potential customers were quite pleased with the results of tests on small quantities; so we expanded the capacity several-fold. It wasn't until the plant was nearing completion that a quality defect was discovered in large batch testing. The applications people have determined that the catalyst residue is causing poor quality, and now I am involved in developing a process to reduce the residue. I will then have to recommend the process to Engineering who will design a plant alteration and modify the polyethylene plant.

"To design this process," Wayne continued, "I need to have certain information on the catalyst's behavior. I classify this information into two types, 'existing' and 'unknown.' Much of my job involves tracing down existing information. It can come from any one of many sources, to cite a few: past lab work, experience in the engineering and commercial divisions on this or related problems, consultants, or the literature. On this particular problem, we're in a unique spot because this is the first project of this type in our company; so no commercial experience is available. Also the past lab work contributes very little to our knowledge of catalyst activity since this was never a problem before. From what I've seen of the literature, that will not help too much either. We will, however, bring a consultant on polymer chemistry in to bring us up to date on this area of polyethylene chemistry."

As Wayne saw it, the situation as he had described it left him with two alternatives;

1. To design the process on the basis of an educated guess, or
2. To propose some experimental work designed to answer the questions on catalyst behavior.

On this project he did not believe there was much choice between these alternatives. He told Bob Handy, "You don't make guesses when the

investment is \$3 million and you know as little as we do. Educated guessing is for smaller projects where the results of a miss are not as disastrous and where the guess can really be educated."

As the situation appeared to Wayne, they not only had to design some experiments to get the information desired, but they also had to go out and sell one of the lab groups on doing the research. Before taking action and proposing his research program, Wayne reviewed in his mind the various pressures under which he had observed the laboratories operating.

He believed that the applications research group put a constant and very important pressure upon the other labs, especially those associated with the pilot plant, for the technical effort required to produce large amounts of material to be used in applications testing both at the central research labs and in the plants of potential consumers. As a result of this pressure the labs and the pilot plant people tended to make the new product by using the original method of production and did very little experimentation in developing process information.

Counterbalancing this pressure was one applied by Wayne and his colleagues when they suggested experiments to provide the development division with process information. These were primarily data gathering experiments, in contrast to the laboratories' own somewhat exploratory work, or applications research's demands for quantities of material.

The third external force exerted upon the labs came about when the engineering department requested detailed information which they required to design new plants. As a service to the chemical research labs, the development division channeled all of these latter requests through their department because they could answer many questions without involving other central research personnel and even when they did not have the needed data themselves, they knew which groups in the central laboratories would have it and could go directly to this group without bothering other researchers unnecessarily.

Wayne also recognized two patterns of pressure placed upon the research labs from within their own organization. First, laboratory management exerted a pressure on researchers to do broad exploratory work in specified areas. As Wayne commented, "The chemical research groups have a much more specific responsibility than engineering or applications. They are interested in maximizing the company's patent position in a minimum of time. They want to get the initial basic data and leave the detailed data collection to other groups."

The other major pressure was produced by the scientist's background, training, colleagues, and personal goals—all of which directed him to make a "real contribution." Obviously there were many other pressures existing within the labs, both individual and group; however, those noted above were the major ones that Wayne and his colleagues recognized as being important in affecting suggestions for research work.

While thinking through the proposals he might make to the chemical research people on the polyethylene problem, Wayne believed it was important to take into account not only these obvious pressures upon lab personnel but also the reactions typical of the research chemists when faced with these pressures. Wayne knew that in the past two important reactions had affected his suggestions which involved lab assistance. As Wayne put it, "Their first reaction is to minimize the amount of assistance they will provide depending on the degree of friendliness and obligation they feel toward the groups and the individuals involved, the scientific position of the person suggesting the work, and the importance of the question to be researched in light of their other work commitments." Bob Handy felt this statement was realistic because as he observed the interactions of Wayne and other development division staff members, he heard such comments as "We'll take this to the vice president if we have to"; "Inexperience and lack of reputation make it difficult to sell your ideas"; "I want to get to know the men at the pilot plant and establish a group like we had on the last job"; and, "I'm not recognized in polyethylene work yet."

The other reaction to pressure was the tendency of chemical research personnel to consider as second rate and unworthy of their effort tests involving engineering applications or process data collection. Wayne viewed this as a result of pressures from the scientific community to do only "substantial" work and noted that it was most evident in the tendency to view any preplanned program of research with alarm and to consider these as attempts to destroy a scientist's freedom to select his own problems of study. In his words: "It would be fallacious to go to the labs with a detailed program of what they should do and how they should do it. This turns the professional into a technician and blocks any possible two-way communication."

It was with this analysis of the pressures and reactions to them as a background, therefore, that Wayne began his analysis of the specific information he thought it was important to obtain from the labs on the polyethylene problem.

During a week of continuous planning of a research proposal on the catalyst residue question, Wayne and Dr. Side were in close contact with the laboratories. Their first steps were directed toward finding what information was available from various sources, primarily from within the Fort Worth Corporation, but also from consultants. They also listed a series of questions which had to be answered before any of the proposed methods for catalyst residue removal could be recommended. Making every effort not to have their list seem to be a "how-to-do-it" program, they drew up a final list of information which had to be collected. Wayne said, "We go into quite a bit of detailed work at this point for two reasons; we have to be pretty selective about the questions we ask of the labs as we don't have a budget. Also we must have a clear picture of what

we want and why we want it so we can do a good job of selling our program."

Dr. Side suggested at this juncture that Wayne take their version of the research proposal out to various persons familiar with the problem in the development division and have them criticize it and punch holes in it informally before presenting it formally to the group which might do the work.

In determining which group should receive the proposal, Wayne drew upon his knowledge of what all the groups were doing. He told Bob Handy, "We make it a point to get around and see the various lab groups and maintain good two-way communication with them. From this we know the status of the various groups as to work load, work interest, and degree of responsibility to and friendship with the development division. Thus we can base our recommendations on this inside information." For example, during the planning of the catalyst research, Dr. Side suggested that Wayne check both the central research groups and the pilot plant personnel to see what they were doing on the polyethylene catalyst problem and whether or not they had made any informal arrangement as to how much future work they would do.

While they were preparing their research proposal, Wayne and Dr. Side decided that they were ready to push this proposal as far as necessary to obtain the information. Wayne said, "When we are working on a proposal, we make some estimate as to how far and in what direction we should push it. It usually depends upon the importance of the work and the responsibilities of the groups involved. This is not a static process but rather a continuous one over the life of the proposal as the variables change. We certainly are selective in pushing projects very hard, for we realize we don't have a blank check."

One day Wayne invited Bob Handy to take a trip with him over to the chemical research labs. He said, "I need some information on the amount of residue present during a part of the process that we believe to be critical before we make our research proposal. I'm going to try and talk one of the research chemists who is making polyethylene samples into making some residue measurements for me."

Wayne introduced Handy to Bill Narberth, a research chemist, and said, "How about making some residue measures on the next batch you make, Bill?"

BILL: I'm willing but I've just recently made a batch and it looks as though it could be as long as a month before I make another.

WAYNE: That's too long. I need it now. Could you make another batch sooner? You'll eventually need it.

BILL: It would have to be pretty important for me to drop what I'm doing. You know my present job has about the same high priority as the catalyst residue problem. Why do you want it?

Wayne's explanation started a discussion on the validity of the question being asked, a discussion which another research chemist who came into Narberth's lab joined. Finally, the problem of technical feasibility was settled.

WAYNE: Then it's agreed. You'll give me a series of residue measures using the techniques you described on the next batch you make, but you can't see your way clear to making another batch solely for this test.

BILL: That's right, because of the importance of what I'm currently working on. I'll do the measure for you just as soon as I can. However, the next batch might be made a lot earlier than next month. Half of the last batch is used, and I made it only two days ago.

WAYNE: Is there any possibility of getting some work done on the alpha method of catalyst removal? We need some information to complete our alpha study.

BILL: That is one you'll have to ask Bob [*Bill's section head*]. As far as I know we have completed all the work that we are going to do on the alpha method. There are two or three men working on the beta method, and from what I hear, the results look promising.

WAYNE: Thanks for your time and help and when you get anything positive, let me know. Good-by.

On the way down the hall, Wayne said to Bob Handy, "Let me fill you in on the background of how we work with the Laboratory Divisions on problems such as this. At intervals we hold a research meeting of all the men involved on a particular project to summarize what their progress has been and to hammer out some form of overall agreement for future work. Once the future laboratory program has been agreed upon, new problems which arise unexpectedly must receive careful consideration before they can be given a higher priority and replace some other item on the program. A good example of this is the alpha and beta methods of catalyst removal about which Bill and I were talking. Both alpha and beta methods were studied in the laboratory some time ago as possible methods of removing catalyst residues. The work on the beta method was more extensive, but even so, it was not pursued too vigorously, since at the time catalyst residue was not felt to be critical to product quality. Recently, when catalyst residues suddenly appeared to be very important, we carried out an engineering study in the development division comparing the alpha and beta techniques. Our studies indicate that alpha looks more attractive economically, but would require considerably more process development work than the beta method. We plan to back the alpha technique in our memo proposing research, and I'm interested to see where the research people stand. In regard to my success in getting the residue measures, it was a compromise, just as many of our proposals end up. If I didn't like the agreement we reached, I could have done one of several things: taken the proposal one level higher in this division either

formally or informally; taken it to another group or individual who could get the data; or, let the recommendation die. Most frequently the course of action is compromise or informally seeking help from the next level. This again depends upon the importance of the recommendation and the responsibilities of the individuals and groups involved. Compromise was fine on the residue measure because it wasn't that critical. However, on this alpha lab work I'm not satisfied with the way it has been going. Let's go see Bob Roslyn."

Wayne and Bob Handy proceeded to Bob Roslyn's office.

WAYNE: How are you doing? Looks as though you've acquired some new men since I saw you last.

BOB: Yes, we got several new men out of this year's quota of new graduates. I understand that you're staying awake nights on this polyethylene headache.

WAYNE: In fact, that's why I came to see you. We are interested in having some experiments run on catalyst removal by the alpha technique and were wondering if you could spare a man. [*Wayne then proceeded to explain to Bob Roslyn the problem on which Wayne was working and why he needed this data in order to solve the problem.*]

BOB: You know we have not done additional work on the alpha method, but we have several men working on the beta method, and we are quite pleased with the results. Right now, one of our men is just optimizing the variables in the method.

WAYNE: How about work on the alpha method?

BOB: We don't plan any additional work on the alpha method. The results of beta have been very encouraging, and there are too many problems in the alpha method. We think beta is the horse to back; so all our effort will go into that research.

WAYNE: Bob, I would be interested in knowing why you decided to concentrate on the beta method when our engineering and economic studies have pointed out the advantages of the alpha method?

BOB: We are familiar with your economic studies which show the advantages of alpha. However, because of the close timing involved in getting a suitable process for the commercial plant and our limited manpower, we decided it would be more desirable to develop the beta method. It will require considerably less effort and is the method on which we have the most previous experience. Sometime in the future we can do additional work on the alpha method so that, if successful, it could be used in future plant expansions or in new plants.

WAYNE: I can see your point, Bob; however, I don't believe that the development work required would be nearly as extensive as you might think, and certainly the economic advantages are considerable. We are currently preparing a memorandum emphasizing the economic advantages and discussing the process development work that would be required on

the alpha process. We still feel very strongly that the alpha process should be developed further. I think this may be a case where we should get all interested parties together in a meeting to discuss the pros and cons of both methods and be sure that everyone is in agreement that the beta method is the best solution after all factors are taken into consideration.

Bob: We certainly would be willing to discuss this with everyone. Let me know when you would like such a meeting and I'll see that our division is represented.

The discussion left the polyethylene problem and covered several other areas of mutual technical interest before it broke up. On the way back to his office, Wayne said, "It is time the development division took a stand on this problem. When we get back to the office I'll sit down and map out a strategy with Dr. Side for formally presenting our arguments for the alpha method."

During lunch that afternoon, Dr. Cote told Handy, "Depending upon the importance of a recommendation, the staff man of the development division would be willing to take the problem as high as the vice president to obtain a solution. Most of our problems are settled at the first or second level, usually by a compromise or by our success in selling a program to the lab. If we can't get a compromise we take some formal action. This may merely be a memo stating what we want and why. Quite often this has the desirable results of forcing a lab group to do the work to absolve themselves of blame if anything happens because our proposal was not heeded. On other recommendations which are more important, our staff is willing to back it with all their weight. This usually means sending it up through the formal channels until agreement is reached at some level." Wayne interjected, "You see it is very difficult to question another professional man directly without maligning his reputation. We avoid this because we don't want to disrupt our present relations with other divisions. It also takes a long time to go through the formal channels. Since such a step causes a lot of commotion, we are very selective about which suggestions we write a letter on and which ones we are vociferous about in other ways."

In talking with Wayne and other development division members, it became apparent to Bob Handy that certain activities played a prominent role in the division's job but were not directly related to their primary task of "providing a process and its economics." To Bob Handy it seemed that these were supportive roles which were directly improving the efficiency of the development division's interactions with outside personnel. In this instance Bob defined "improving efficiency" as maximizing the chances for acceptance of recommendations while minimizing the conflicts and disturbances created. He was certain that these activities were not done solely to improve division efficiency, and it was not apparent that efficiency was a conscious justification in any or all of these

activities. However, Bob was sure that they were important to the division's success in its prime task.

For instance, all during the week he was preparing the development division's formal proposal on alpha method of catalyst research, Wayne was constantly being interrupted by people located in technical groups elsewhere in the laboratories asking him to answer questions or provide information. Wayne commented, "As the central group in chemical R & D we probably have the greatest density of information on any product in the development stage. The development division gains an early familiarity with a project and we make it a point to stress our information service. If we don't have the information, we usually do know the best place to procure it. This helps us quite a bit in our relations with the other divisions, for they feel obligated to us and are much more receptive to our suggestions. Having this information allows us to perform other services which also obligate the divisions and help improve our reception."

As another example, Wayne underlined the service the development division performed by channeling engineering's requests for information to the chemical research labs, since much of the desired information was already available there and men in the development division knew the location of it. They were thus in an excellent position to serve both engineering and research. If there were any requests for unknown information, Wayne appraised the request, developed a program to provide the answer and suggested the program to the appropriate group. As an example he said, "When, or if, this polyethylene catalyst removal process ever gets to engineering for plant design and they run into a problem, I'll get the call. If I can't answer it from my own background, I'll know the right lab people to contact. The research labs instigated this program of using the development division as a buffer and seem to appreciate our efforts."

Wayne mentioned several other services the development division performed in their effort to maintain good relations with other divisions. "One of our big problems in this polyethylene work is that I have no established position of scientific prestige or reputation. In the past three years our top specialty products men have been transferred out to other parts of the company and with them went the prestige of the section. Dr. Side and I plan to develop our position by making some positive contributions on some other problems in polyethylene. This helps a lot when we have an idea to sell. In new areas such as this we will try to borrow prestige from persons already established. What we are trying to do is get the weight of informed scientific opinion on our side in this case. In fact, Dr. Side and I have a meeting scheduled with the man from engineering who designed the plant to get him to back our ideas on the alpha method."

"While we are on the subject," Dr. Side commented, "we might as well mention the value of the project groups in our lab relations. Poly-

ethylene is new and the group is not yet well established; in fact, that is one of Wayne's major tasks in the near future, the formation of a good communication network including every group or person intimately involved in the work. It is nothing formal; we had it on the last project and it worked fine, improved communications and relations with the labs. Wayne plans to spend some time with the pilot plant group working on establishing a mutual background on polyethylene. It works just as if we actually had a project group except there is no red tape or supervision. Again Wayne's success depends a lot on his scientific position and friendship with the lab personnel."

As the date approached for the formal presentation of the research proposal on the alpha method, Wayne grew more nervous. He said, "Sometimes I wonder whether we have done all that is possible to gain an easy acceptance of our recommendations. At other times I think that a group such as the development division should have authority comparable with its responsibility." Bob Handy asked Wayne what he meant by "authority comparable with responsibility" and Wayne replied, "One can be all things to all men, which in the long run is an impossible course of action because one must, at some time, take stands on particular issues. On the other side of the picture, you might take stands on everything. Quite often, nevertheless, what we need is an authoritative decision rather than a democratic discussion at lower levels. An authoritative decision causes no more discomfort than a democratic decision and it takes much less time."

Chapter 22

THE HOUSTON CORPORATION (A)*

IN THE late spring of 1959, Bob Dixon[1] was completing a report which had been requested by top management of the Houston Corporation's research and development laboratory. Dixon had been asked to review and submit recommendations to management concerning the policies and procedures currently in use in regard to the recruitment, selection, and hiring of professional employees for the various operating divisions of the laboratory. Specifically, he had been instructed to report briefly how the present system operated, the problems he saw, and the solutions to these problems he recommended.

Dixon was a member of the employee relations staff at the R & D laboratory and before taking on this assignment had done several other staff studies involving research personnel. Three years before he undertook the present assignment, he had been intimately involved himself in college recruiting for the research and development laboratory.

The Houston Corporation was one of the nation's largest producers of chemicals. Products ranged from basic chemicals, explosives, and fertilizers to consumer products such as synthetic cloth. All of the research, development, and engineering for Houston's commercial divisions was performed at the research and development laboratories (Exhibit 1). The total research staff numbered about 2,800 people with an annual budget of $35 million.

The annual recruiting quota of the laboratories varied between 80 and 175 college graduates, depending upon current business conditions, and the job of coordinating and controlling the search for these men fell to the employee relations department at the labratories. The employee relations staff consisted of 22 men with a professional interest in personnel work. Most of them had training in psychology, industrial relations, or other forms of business administration. As may be seen in Exhibit 2, only about nine of these men were directly involved in the college recruiting program. Employee relations reported directly to the administrative director of the research and development laboratories.

As the first step in his assignment, Bob Dixon prepared a detailed report of the steps through which a college candidate was processed. He also

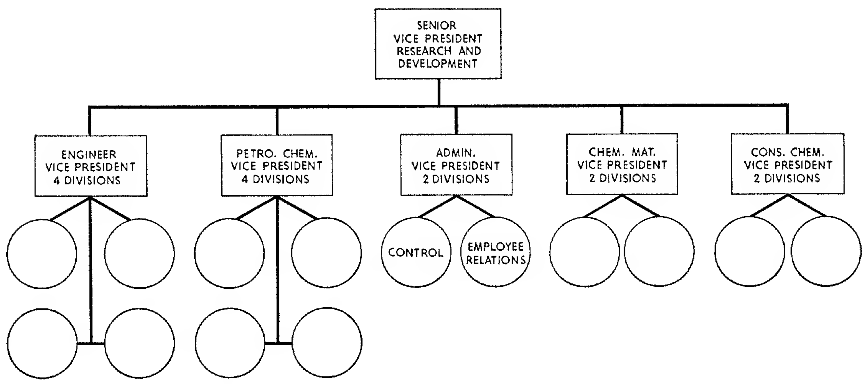
* Copyright, 1959, by the President and Fellows of Harvard College.

[1] All names disguised.

Exhibit 1

THE HOUSTON CORPORATION (A)

RESEARCH AND DEVELOPMENT LABORATORIES
ORGANIZATION CHART



drew a simplified flow chart of these steps (Exhibit 3). A digest of Dixon's report follows. (Numbers in parentheses refer to steps as numbered on the flow chart.)

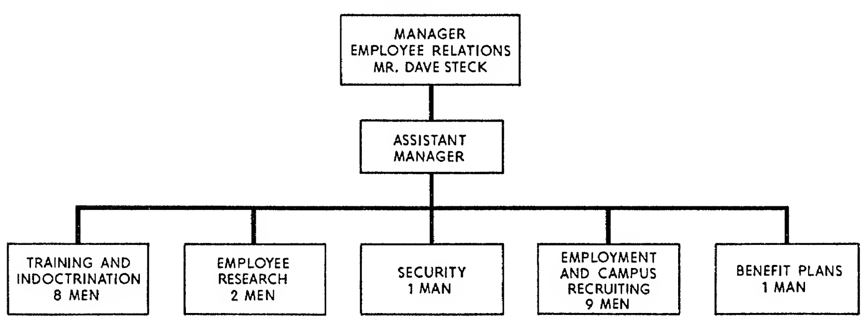
Planning Phase

Prior to any particular year's recruiting activities, each division determined its quota of new hires for the coming year on the basis of the growth allowed within its budget plus expected attrition of present personnel during the year. Once the recruiting goal had been established, personnel from employee relations determined the size and cost of the program necessary to fill these quotas. Then a program listing the schools to be visited was presented to the divisions; usually there were only minor

Exhibit 2

THE HOUSTON CORPORATION (A)

ORGANIZATION CHART OF EMPLOYEE RELATIONS



changes from year to year in this list depending upon the availability of campus contacts and past results from any particular school. There were usually one or two formal meetings conducted by the employee relations group to inform the interviewers of this year's program and goals. For new interviewers there was some formal training conducted by employee relations.

Campus Phase

The next phase was the long period of campus trips scheduled throughout the fall and winter. All campus recruiting was done by research personnel from the divisions.

The researchers' duties in connection with the campus interviewing program fell into two broad categories. The primary job was that of the "campus contact." There were 100 professionals in the laboratory who were assigned on a part-time basis as campus contacts. Most of these men performed this service for one school, and if at all possible, the individual appointed would be an alumnus of the school who had fairly close ties to the chemical and chemical engineering faculty of that particular college or university. Ordinarily, the campus contact would visit his school from one to five times a year and would spend as long as two weeks per visit talking with faculty members about likely candidates and interviewing these candidates.

The campus contact group visited 125 schools every year (some of the smaller ones were visited by men who also had responsibility for a larger school in the same geographical location). In each case the campus contact had primary responsibility for maintaining close professional liaison with the school and also served as the primary interviewer.

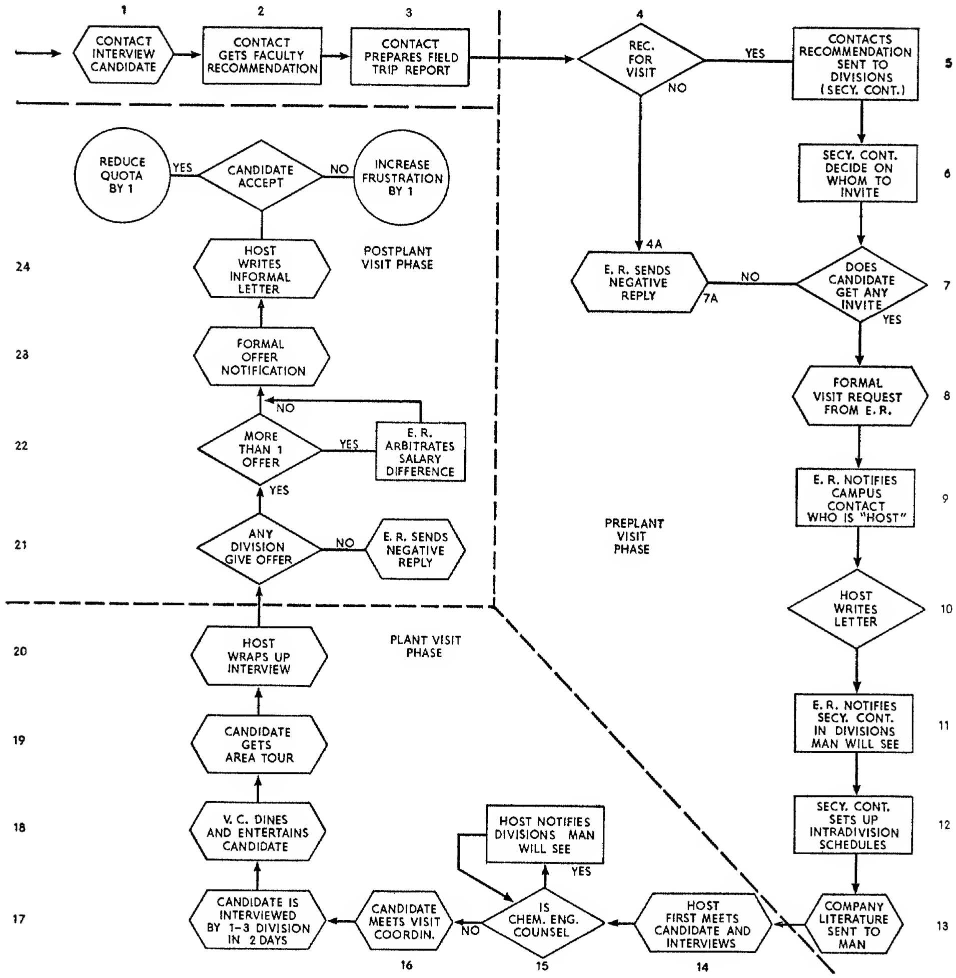
At most of the larger universities, the interviewing burden became too great for one man to handle. In such cases the campus contact would be assigned one or more "campus interviewers." These men were also research personnel from the labs, usually younger or with less experience and often under consideration for eventual assignment as a campus contact. Working under the supervision of the campus contact, these men would share with him the interviewing burden.

During the interviews, the Houston men try to provide the students with information about potential positions in the company and the laboratories (1). Simultaneously they are making a preliminary appraisal of the student based on information he has given them and their own judgments of the man. A field trip report covering each man interviewed must be turned in by the contact (3). These reports include ratings of personality and intelligence of each student interviewed, recommendations that some of the students visit the plant, and recommendations concerning particular research divisions which might be interested in seeing the man. All information obtained from the school's files or the professors' com-

Exhibit 3

THE HOUSTON CORPORATION (A)

FLOW CHART OF RECRUITING PROCESS
CAMPUS PHASE



ments must be included in this report (2). The campus contact also stipulates men to whom negative replies should be sent and gives reasons justifying this action (4).

Preplant Visit Phase

Once the field trip reports are complete from a particular campus visit, they are sent to employee relations; here the students with negative comments are sent negative form letters (4a). On the positive recommendations, the trip report is reproduced, and one copy is sent to the

"secondary contact" in each laboratory division (5). This is a technical person, usually in a supervisory position, who holds the primary recruiting responsibility in that division; he is the key communication link between employee relations and the research staff. The next step is the selection of candidates in whom particular divisions are interested (6). Each division has different methods and personnel involved in making this selection; there is very little uniformity except in the opinion shared by all divisions that they should do a better job.

When employee relations receives from the divisions the lists of men who interest them, they send form letters to the candidates the divisions indicated, inviting them to the laboratories. If any candidates on the field trip "recommended list" are not invited by any research division, they are sent a negative letter similar to that resulting from the field trip negative list (7a). At the same time, employee relations notifies the campus contact or the interviewer who talked to the men, which candidates they interviewed are being invited to the labs and which divisions are interested in these men (9).

The campus contact then assumes a new role as "host" for those men he originally interviewed. The first of his hosting duties is to write the candidate an informal, friendly letter seconding the invitation to the labs and telling the student that he will be his host during his visit (10). All the details of the visit are handled by employee relations, with the exception that if the candidate brings his wife, it is the host's duty to see that she is entertained while the man is visiting the laboratories.

At this same time a "visit coordinator" is assigned to each candidate coming to the lab. This is usually a young technical man with the same educational, marital, and interest background as the candidate. He is selected by employee relations and may be asked to serve in this capacity three or four times a year.

As soon as the candidate accepts the invitation to visit, a schedule is established for him, and the divisions which evidenced an interest in seeing him are told when he will be available (11). This information is communicated to the secondary contact, whose responsibility it is to develop the man's intradivisional schedule (12).

Plant Visit Phase

The plant visit, which is usually two days long, begins when the candidate first meets his host. It is the host's duty to see to it that the candidate's living accommodations are adequate and to make the candidate feel at ease at the start of his visit. During the host's introductory interview with the candidate, he orients the candidate to the company and to the labs, explains the remainder of the visit schedule, and checks to see that the divisions which the man will interview are ones in which he is interested. Candidates who are chemical engineers run into a unique

problem; all 12 divisions usually want to see qualified chemical engineers. Therefore, the host performs functional counseling for the chemical engineering candidates, determining in detail what their interests are and advising them which divisions to see (15).

During his two-day tour, a candidate can see three divisions if that many indicate an interest in him. At the end of the host's interview the candidate is introduced to his visit coordinator, who has formal responsibility for guiding the candidate between divisional interviews and entertaining the candidate. It is impressed upon the candidate that the visit coordinator is not evaluating him and is free to answer any questions about the labs. It is hoped that during the time the candidate and the visit coordinator are together between divisions, at meals, in the evenings, or on the area tour, the candidate will ask any questions that may occur to him and recognize that he is getting frank answers (19).

After the man has seen the divisions that expressed an interest in interviewing him, he has a final or "wrap-up" interview with his host. The purpose of this is to provide him with information about company benefits and answer any final questions; also, if any divisions have committed themselves already, the host tells the man he will receive an offer. Usually it is only to superior candidates that an offer is mentioned during the wrap-up interview.

Postplant Visit Phase

The host tells the majority of candidates during the wrap-up interview that they will soon hear from the company. Employee relations checks with each of the divisions the man has seen and determines whether they want to make an offer and what salary they are willing to offer (21). Normally, most divisions take some time to make a decision on the average man, while decision making on the exceptional men or the low-ranked men is easy. When two or more divisions decide to make an offer to the same man at different salaries, employee relations arbitrates between the divisions to arrive at a common salary level for both offers. Employee relations then sends a formal job offer letter (23), which is accompanied by an informal letter written by the candidate's host (24). It is the host's duty to maintain contact with the man until he has either accepted or rejected the offer.

* * * *

While the foregoing description of the recruiting program outlined in a formal way the details of the process and the relationships required for its implementation, it was clear to Bob Dixon from his experience with the program that there were many informal relationships and many problems in connection with them that had to be taken into account before he could make helpful recommendations to management. In order to clarify these in his own mind, he decided to interview technical men who were

members of the research staff and various interested people in the employee relations department. The remainder of this case is a summary of his notes on these interviews.

Interviews with Campus Contacts

Houston has about 100 men in this category, yet it is difficult to build any image of the typical contact; they vary from older experienced men who maintain close contacts at their alma mater to young engineers recently assigned to handle recruiting at a school they have never seen. The experience, ability, and interest also vary widely among the contacts. Usually, they visit a school only once or twice a year on their recruiting trips, spending about a week per trip; this breaks down into two days for interviewing and three for traveling, meeting faculty and placement officials, and obtaining information on candidates. Most of the campus contacts feel they should spend more time developing faculty contacts and beating the bushes for the top men, but they just don't have the time. What worries these men the most is how to do a good job in this work without neglecting the technical jobs on which their performance is rated.

While operating as an interviewer on campus, these men develop their own policy of what to say and do in many instances. Though most of the contacts gripe at the time consumed by this job and their complementary job of being the host to the men they interviewed, none of them wanted a change in the system which would leave them out. They all valued the opportunity to visit the university at least once a year. They felt their jobs were improved by the faculty contacts, and they valued the impact of a change of pace and of perspective a university system provides. They have problems with some of the minor aspects of recruiting, for example, deciding when to invite the candidate's wife. They feel that Houston should show more confidence in them by allowing them to invite candidates to visit the laboratories during the campus interview, but on the whole, they enjoy the job they are doing.

Interviews with Secondary Contacts

These are the men in the divisions (one per division) upon whom the major burden of the recruiting task falls. They estimate that they spend from one fifth to one half of their time on recruiting problems. In most divisions, the secondary contact job is rotated every year in an effort to ease the burden on individuals and also to develop some managerial and administrative talents among the technical supervisory staff. Most of these men are in junior administrative positions, and in many divisions, their normal technical responsibilities are relieved to some degree during their stint as secondary contacts. It was the consensus of opinion that the work

could be divided into three areas: (1) deciding which candidates to invite for a plant visit, (2) scheduling of candidates through the divisional interviews, which includes decisions as to whether or not to hire, and (3) handling all communications and interactions with employee relations. In every division, secondary contacts felt they had the major recruiting responsibility and in many divisions, most of the authority as well.

When the field trip reports come in from the campus contacts, secondary contacts determine which candidates their division shall invite, based upon the interviewers' recommendations, faculty ratings, and the man's scholastic background. In most divisions the secondary contact does this alone, but in some he is a member of a team which decides.

Most of the secondary contacts interviewed believed that the recruiting system was weak at this point—that is, the point where a decision is made to invite candidates to visit. The reason for this, they felt, was that Houston's system of using 100 technical men as "amateur" interviewers (campus contacts) did not provide them with the quality of information necessary for making such decisions. They believed a small group of competent professional interviewers could do a better on-campus job. They mentioned, for instance, that, with the exception of the older, more experienced campus contacts, they had no idea what standards the interviewers were using and how these standards compared with their own. They believed that if fewer campus contacts covered the various campuses on a full-time basis, there would be a better opportunity for the secondary contacts to get to know these men and correlate their judgments on candidates with their own.

The second major complaint the secondary contacts mentioned in regard to the campus contact system was that most campus contacts have little knowledge of the positions available in any division except their own. Therefore, the secondary contact cannot use with confidence the contacts' recommendations as to which divisions the man should see. To avoid missing any candidates because an unknown contact gave them only a fair recommendation or did not recommend that their division see the man, the secondary contacts are lenient in their standards regarding which men to invite. They expressed the belief that, given a smaller group of professional interviewers whose recommendations they could believe and utilize, the number of candidates invited to the labs could be reduced substantially.

The second major function of the secondary contact is coordinating and scheduling the intradivisional interviews. Interviewing policy within the divisions ranges from very informal chats with available staff members in a small division to the formal and very elaborate interviewing system (described in a 100-page booklet) used by one division staff. The typical division interview is scheduled by the secondary contact or his secretary as soon as notification is received that the candidate will see their division. A typical schedule for the three-hour interview includes talks with four

staff members ranging from a newly hired professional, who describes a possible first job, to the division director, who discusses how the candidate might grow in the division and the company. For candidates with especially good prospects there is often a "red carpet" treatment, which includes extra time with the director and persons working at the bench level.

As the candidate progresses from interview to interview, the secondary contact gathers together the successive interviewers' evaluations of him and, at the end of the candidate's visit, presents these at a meeting of division management. At this meeting a decision is made on whether or not to make the candidate an offer and, if so, how much to offer him.

Finally, the secondary contact is responsible for maintaining communications between employee relations and the staff members in his own division who are involved in recruiting. It is only through the secondary contact that employee relations can apply pressure to the campus contact who is not performing well or pass along recommendations to all contacts concerning improvements in campus recruiting.

Interviews with Employee Relations Personnel

The entire responsibility for the coordination and actual operation of the recruiting procedure falls upon a small group of men in the employee relations division. Their outlook on the problem is entirely different from that of most of the technical men whose ideas have been summarized above and who, generally speaking, consider the program to be a necessary evil tolerated because of the evident need to be able to get good men. While talking with employee relations men, it appeared obvious that they are proud of the recruiting and hiring system and the operation of their group. They believe that, in comparison with other competitive recruiting operations, their correspondence handling and in-plant visit operation particularly are better handled. While they feel that there is always opportunity for improvement in these areas, they believe the changes needed are primarily of a procedural nature rather than any basic revision of policy.

There are, however, two areas of serious concern the employee relations men have mentioned. One of these has to do with the campus recruiting procedure and the other involves the divisions' evaluation of candidates. In the area of campus recruiting, they find it very difficult to communicate with over 100 campus contacts and an additional 50 campus interviewers. The men in employee relations feel that any streamlining of the program which would reduce this number would be an improvement; but, at the same time, they recognize the arguments for maintaining the status quo. For instance they see two advantages to using research personnel as campus contacts. First, these men can engage in technical discussions with both faculty and students at the various colleges they

visit, and this is considered an important asset which would be lost if professional interviewers were employed. Second, they feel it is important to spread the interviewing burden among many men to avoid having anyone develop a "canned" talk. Several employee relations staff members mentioned that in comparison with the program of some of the leading competitors for technical talent, Houston has by far the most decentralized and least "professional personnel man" oriented program at the campus level. They feel this is advantageous because the program gains immeasurably from the involvement in it of researchers and research management.

On the negative side, it is felt that during the plant visit many of the divisions duplicate efforts to evaluate the candidate. Some of the employee relations personnel are looking forward to the day when a candidate may be evaluated once by a central group composed of technical men representing several divisions and the decision to hire or not would rest with them. This would probably reduce greatly the amount of time a man would spend on a plant visit, even possibly cutting it to one day. Many of the personnel in the divisions and in employee relations agreed that such a plan would be beneficial, but the latter questioned whether the divisions would be willing to relinquish their authority to this extent. It was felt that a central group with practice and training could improve the selection procedure and the quality of men selected; however, they would have to be aware of the varying standards and criteria which were important to each division.

Generally speaking, the employee relations staff group believed that their greatest single problem in regard to the recruiting program lay in their inability to resolve many of the smaller problems, such as changing the wording of letters to candidates without first getting the approval of each division. Their experience had been that the divisions would not yield to them the authority to make minor changes such as these because they felt this would infringe on the division's ability to select good men.

Interview with Dave Steck

As a final step in preparing himself to make recommendations to management, Bob Dixon felt he should check on some questions of basic policy with Dave Steck, the manager of employee relations. His conversation with Steck went substantially as follows:

BOB: How did this department form, Dave, and how do you envision the direction in which it should progress?

DAVE: This is a rather new department at Houston labs, having been formed only five or six years ago when the various divisions gave up trying to compete with each other on the campus for talent. I shouldn't

say they gave up, because they still are competing; however, many of the jobs which were originally performed by each division were centralized in employee relations in the belief that we could accomplish them more efficiently. That essentially is the story of this department's growth. At first we centralized only a few of the glaring inefficiencies, but gradually we have been adding more and more to our duties, and I should add, to our authority. If you asked me to predict the future, I would guess that this growth trend will continue rather than halt, for two possible reasons. First, we are continually learning more about this business of recruiting, both through practical experience and trials and errors, and through employment research, of which we are doing more all the time. We are trying to decide what really are the characteristics of the men we want in

Exhibit 4

THE HOUSTON CORPORATION (A)

RESEARCH AND DEVELOPMENT LABORATORIES
REPORT OF CAMPUS RECRUITING RESULTS FOR 1959 SEASON

	Chem.	Ch. E.	M. E.	C. E.	E. E.	Other	Total
Contact Status							
Trips completed	...	...	...	...	...	...	164
Trips remaining	...	...	...	...	...	...	0
Number men interviewed	630	1,022	451	488	211	344	3,146
Number men invited in	344	547	210	146	83	114	1,444
Invitation Status							
No reply	1	2	2	1	0	0	6
Refused	60	99	61	30	33	24	307
Undecided	3	0	1	0	1	0	5
Came in	276	444	146	114	48	90	1,118
Still to come	4	2	0	1	1	0	8
Offer Status							
Offers made	173	322	87	52	39	57	730
No replies	11	18	4	1	0	3	37
Decision later	10	13	1	0	0	1	25
Refused	115	196	58	30	33	34	466
Accepted	37	95	24	22	6	19	203
Target (would like to hire)	42	123	48	24	12	12	261
Percentages							
Trips completed	...	...	...	...	...	...	100
Invited/interviewed	54	53	46	29	39	33	45
Visited/invited	80	81	69	78	57	78	77
To come/invited	1	0	0	0	1	0	0
Offers/visited	62	72	59	45	81	63	65
Decisions/offers	87	90	94	98	100	92	91
Accepts/decisions	24	32	29	41	15	35	30
Accepts/target	88	77	50	92	50	158	78

NOTE: It must be remembered in viewing these results that Houston's recruiting goals in 1959 were especially high in comparison with normal years.

Houston's labs and how best can we get them. This increased knowledge of recruiting, its rationale and methods, will automatically give us increased responsibility and authority. Second, management has a growing desire to remove the scientists from administrative work whenever possible and give them more scientific work. The central position of employee relations has been growing and probably will continue to grow; what we are trying to reach is a balance of power with the divisions.

BOB: I would think that in a position such as yours, top laboratory management would be most receptive to any idea if you could demonstrate performance improvement.

DAVE: This is one of our most serious problems, measuring our own performance. How do we go about doing it? The furthest we have gone is to show how we do statistically, but is quantity all that counts? Here is our record for this 1958–1959 recruiting period which shows our score in each performance category (Exhibit 4). This allows us to make comparisons between competitors and ourselves, and also over time. However, two important items are missing, namely, the quality of our new hires and the number and type of men who never sign up for a campus interview. Both of these are difficult to measure, as we have recently found out, and we are not at all sure that our program of recruiting as now devised is bringing us the quality of hires, particularly PH.D.'s, that some of our competitors are able to attract. These are merely a few of the problems on which I would like to see more employment research done. If it is done, we will be in a much stronger position than our competitors in this manpower race.

BOB: Thanks for giving me your time and this information. I think it is quite important for me to adjust my recommendations to the policy and power framework existing, and what you have told me will help me do that.

Chapter 22 (Continued)

THE HOUSTON CORPORATION (B)*

THE FOLLOWING report contains Bob Dixon's[1] recommendations for changes in Houston's professional recruiting program.

PROFESSIONAL EMPLOYMENT: A REAPPRAISAL

Earlier this year the administrative committee asked that I study and recommend improvements to the company's professional employment program. Two questions were suggested for investigation:

1. How well do we handle candidates from first interview to final offer decision? Can the program be better organized to make it run smoother and increase the percentage of acceptances/offers?
2. Are changes necessary to improve the job on that small fraction of exceptional PH.D.'s?

This memorandum reports recommendations in the first of these areas, while work on the second question is under way.

Recommendations Based on Internal-External Studies

New hires and old employment personnel in Houston labs (secondary contacts and long-tenure campus contacts) were probed for the benefits of their experience. For another perspective, visits were made to seven manpower competitors and three universities. From this three-pronged study evolved recommendations which, after modification, were accepted by the technical divisions. These recommendations will result in operational changes on campus and during the plant visit.

Campus Problem: Responsibility Too Diffuse

Houston labs have 102 campus contacts; the major competition, several with higher quotas, depend on much smaller interviewing staffs. The larger group is regarded as undesirable . . .

. . . because it is impractical to orient properly so many people, most of them making only one or two trips per year on the company's needs. Therefore,

* Copyright, 1959, by the President and Fellows of Harvard College.

[1] All names disguised.

they go out without really knowing in thorough detail who the company as a whole wants to hire.

. . . because the man who makes one or two trips a year just can't develop the skill acquired by the "professional" who makes a dozen trips.

. . . because program changes can't be made with any speed; the communications problem is awesome.

. . . because the really effective campus man is a rare item; by relying upon too large a force, many less-than-superior persons are included.

. . . because the man with one or only a few schools tends to give this small part of his total job a lower priority. With today's competition nothing less than intensive concentration on the job will pay off.

All of these ills are easily identified in Houston's program. It is therefore recommended that the 102 interviewers be reduced to a carefully selected 25–30, 11–15 for BS/MS trips and 14–15 for Ph.D. trips. The exact number will be the minimum necessary to do the job well without committing any individual to more than two to two and one half months of this "nontechnical" work.

These "experts" would serve at least three to five years, continuity being an aid to campus relations building. Each would commit eight to ten weeks a year to the program, including time for travel, interviewing, getting faculty evaluations, building campus relations, and hosting. (Over all this is about the same time as spent by the 102. Though this change effects savings in travel time this saving is being used for "faculty contact time." On each trip the interviewer will now spend an extra day talking to the faculty.)

Two Phases to Be Separated

It is recommended that the BS/MS and Ph.D. operations be separated. Different techniques and different types of interviewers are needed. Therefore a select team will handle the 40 Ph.D.-only trips and host the candidates brought in from these trips. (Note: About one third of the Ph.D. visitors will come from the 100 BS/MS trips; these candidates are primarily at schools where a separate Ph.D. program is not merited by the normal quality and quantity of the graduates. These Ph.D. visitors, as at present, will be hosted by their BS/MS interviewers.)

Shift in Orientation and Follow-up Responsibility Recommended

Concentration of campus interviewing along the lines recommended above has yielded big campus dividends for our competition, but it necessitates a change in the in-plant visit phase. Under the old setup, with the larger team, each interviewer easily handled the nontechnical phases of his candidates; visits—describing the company, benefit plans, etc.; "wrapping up" at the end of the visit; and following up periodically after

the offer was made. But with a concentrated campus effort this becomes more difficult:

> . . . Overall, the time consumed on these hosting activities equals the visiting campus time. The interviewers are just as valuable to operations as they are to employment.
>
> . . . It is unfair to keep the interviewer out of the technical mainstream any longer than is absolutely necessary.
>
> . . . Keeping the candidates' visits from interfering with the interviewers' campus trips creates scheduling problems.

Other companies, therefore, have seen fit to turn this orientation and follow-up function over to the central employment group. The divisions agreed to follow this plan with the BS/MS candidates. The 11 to 15 campus interviewers will turn over their candidates to the employee relations staff who will assume the hosting, wrap-up, and follow-up responsibilities. This will free one year of technical time in the divisions and add the same amount to employee relations workload. Employee relations will distribute this function among four people so that each candidate will get the individual attention he deserves.

It should be noted that our competition has realized gains from this change born of necessity:

> . . . As with the smaller campus force, "professionalization" results in a far better job being done.
>
> . . . The employee relations group has a personal acquaintance with every candidate thus acquiring a more intimate involvement with the lifeblood of the program.
>
> . . . Changes affecting visitors or outstanding "offerees" can be implemented immediately, since the contact personnel are within the employee relations group. When the hosts are scattered across the labs there is a very little flexibility.

Ph.D. candidates will be handled as in the past with the technical men doing the hosting; the lab divisions feel that the Ph.D. interviewer will have built up a relationship with the candidate over the years so that it would be inappropriate to bring a new man in as host.

Communications and Time Lags to Be Reduced

Because they could not repose full confidence in the varying standards of 102 interviewers, the divisions have been making case-by-case decisions on (*a*) which candidates should be invited, and (*b*) which divisions each candidate should see. The result has been an elaborate communications problem and extra weeks of lag time during which candidates have been lost. Use of a professional campus force should make it possible to rely with increasing confidence on their judgment on whom to invite. Also, data furnished by a better-informed corps of professional interviewers will enable employee relations to schedule candidates to the divisions more

effectively. This will obviate the unwieldy current system of polling divisional interest in each candidate.

Conclusions and Recommendations

A study of competitive practices shows that our professional employment program is not organized for optimum yield in today's technical employment market. Therefore, these recommendations are offered:

1. Separate BS/MS from Ph.D. hiring.
2. Reduce the campus contacts from 102 to 25–30.
3. Transfer responsibility for candidate hosting, wrap-up, and follow-up from the campus contact to employee relations staff people for BS/MS candidates.

Chapter 23

AMERICAN METALS COMPANY (A)*

INTRODUCTION

THE AMERICAN METALS COMPANY was one of the largest diversified metal companies in the world in 1963, with annual sales well over $1 billion. The company produced over 5,000 products, which were used by almost every manufacturing and processing industry, and employed about 50,000 people at more than 200 plants, offices, laboratories, mines, oil and gas properties, and quarries in the United States. Some 2,000 scientists and technicians were engaged in investigation of new products and processes. Research expenditures for 1963 were budgeted at $40 million.

Like many of today's large companies, AMC began in the twenties as a financial consolidation of six unrelated metal companies. These companies retained much of their original character when they became autonomous divisions in the newly formed AMC. While none of these companies originally competed with each other to a significant degree, there was in 1963 a marked tendency toward interdivisional secrecy and competition.

The divisions were responsible to corporate management only for their P & L statements. There had been some attempts by corporate management to centralize and coordinate divisional activities, but this had been met with resistance from the divisions who wanted to retain their autonomy.

One of the results of corporate managements' desire for coordination and control was the creation of the Central Research Laboratory (CRL) shortly after World War II.

While each division had its own research function, divisional research tended to be product-oriented and defensive in nature. This was caused, in the opinion of several executives, by the impact of divisional research expenditures on the divisional P & L statements and the strong pressures exerted by the marketing functions for solution to "brush fire" situations. Recognizing these factors, corporate management set up the CRL in order to increase the company's effort in offensive research.

While the CRL's primary mission was to engage in offensive research,

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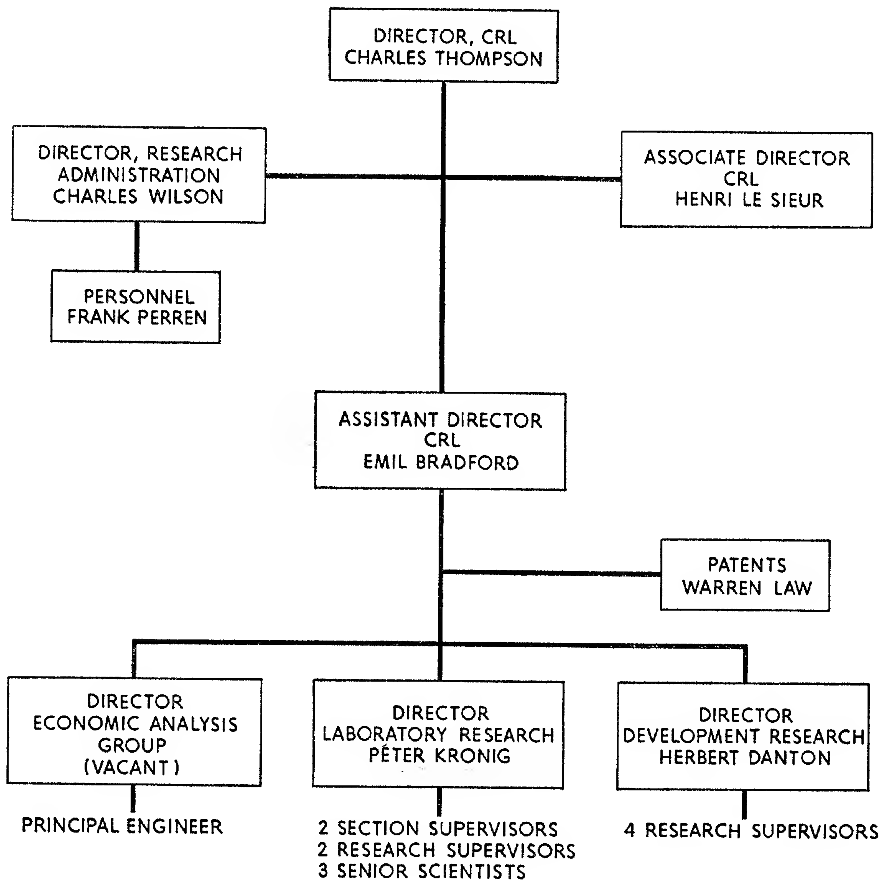
it also did defensive research and service work for the divisions when the divisions lacked the technical competence.

In the case of exploratory or offensive research, the CRL had to "sell" the concept, or product, or a process to one or more divisions. A division did not have to accept the responsibility for a product or process if its

Exhibit 1

AMERICAN METALS COMPANY

PARTIAL ORGANIZATION CHART, CRL



marketing management did not feel that it would "go." The successful completion of a research project for the CRL was, in one sense, therefore, acceptance of it by divisional marketing management.

The CRL had over 300 employees, more than half of whom were professionals. The laboratory was housed in several new buildings in a rural setting near a large midwestern city. (Exhibits 1 and 2 show partial organization charts of the CRL.)

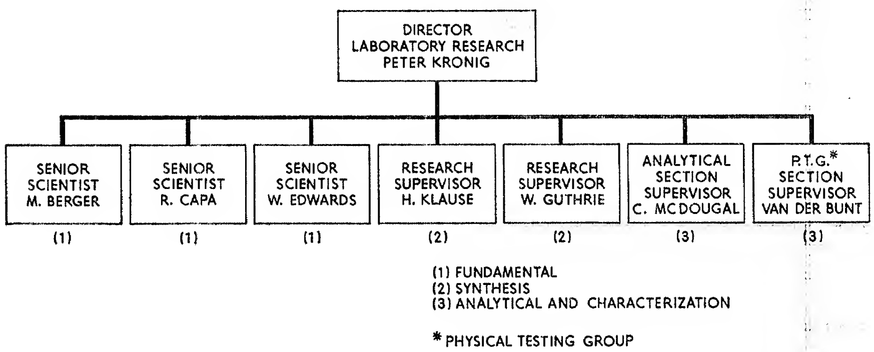
A casewriter who spent some weeks observing activities and interviewing personnel at the CRL found himself particularly interested in the manner in which the research done at the laboratory was (*a*) initiated and (*b*) evaluated. The remainder of this case deals with these aspects of the research and development process.

During the first few days of his period of observation at CRL, the casewriter spoke to Charles Thompson, who had been promoted to the position of director of the laboratory six months before. Thompson told him: "Ninety-five percent of the work done here initiates within the CRL. A research idea or project usually initiates from the man at the bench. This is to be expected because the man at the bench has the most competence in his field. We try to encourage a stream of ideas by giving

Exhibit 2

AMERICAN METALS COMPANY

PARTIAL ORGANIZATION CHART, CRL



the man at the bench the freedom to produce new ideas. Further, we try to maintain an atmosphere which will permit a man to continue on his project for some time even if his supervisor and research management feels that project has no potential for the company."

Dr. Thompson went on to say that this freedom had paid off in several instances with highly successful projects.

Emil Bradford, the assistant director of the CRL had this to say about the evaluation and selection of research projects: "One conclusion we have come to is that the selection and evaluation process is very complex and can not be quantified. Evaluation and selection are really dependent upon the element of judgment. We have to rely on the judgment of research managers, and the only way we have of appraising this is according to the number of successes research management has had in the past."

Dr. Bradford went on to describe the process of evaluation of ideas and projects as continuous but with shifting criteria: "The initial idea usually

comes from the man at the bench. He will inform his supervisor and may do some preliminary work simply on the basis of interesting metallurgy. If the idea shows some promise, the supervisor will inform the lab director who will then request the economic analysis section to do a preliminary informal report on the economic aspects. This report will give a rough guideline of the consequences of success, using criteria such as the company's raw material position, potential market, and so on.

"This report helps the laboratory director in evaluating the research idea. A decision is then made by the director of the CRL on whether to proceed on a project and at what pace. The criteria at this point are the judgment of research management and the overall position of the CRL. If the decision is to go ahead, personnel may be shifted to the new project and a project manager is assigned if it is going to be a major effort. By this time the project is probably in the development stage and a great deal of thought is given to transferring the project to a division. Since we have no marketing function at the CRL, a project is successful only if it is "sold" to a division, and the criterion of "sellability" is certainly important in evaluating our research."

Dr. Bradford continued: "The divisions evaluate the projects on their own. They don't have to take the market predictions or cost figures we give them at face value, but our aim in giving them these data is to prevent duplication of work in the future. We are going to try to get in touch with the divisions at a much earlier stage. We want our economic analysis section to work with the corresponding people at the divisional level on an informal basis to get quicker feedback on how the divisions feel about our projects. We have been looking for some time for a better way to evaluate our research program. We feel there must be a way to select projects with a higher rate of success. Considering research from initial idea to completed project as a process, there are fewer failures at the tail end of the process, but as the cost of each project is higher at the end, each failure there is much more expensive. So, the total possible loss is higher at the development stage than at the fundamental stage. Therefore, there is a much greater need to control the project selection at the tail end of the research than at the beginning."

Talking about the economic analysis section Dr. Bradford said: "As you can see, we have no director of economic analysis right now. We are trying to get the best man for the job and we have to look outside the company for him. We need someone with a marketing and business background. We are already very strong on the manufacturing and engineering aspects; that is, the cost of a new plant, the cost of producing a material, and so forth, but we need to have a much stronger feel for marketing. We have to facilitate communications with the marketing groups in the divisions because we are depending to some extent on the marketing people in the divisions for our success."

When asked to describe the difference between lab research and

development research, Dr. Bradford said: "The lab research group does research on the basic level. This means it does mostly fundamental and exploratory research. The development research people do some applications research and some scale-up. We have to have a development department at CRL and we have to do applications work in order to sell a particular project to the divisions. We try to interest the divisions as early as possible and it's usually after some applications work has been done. Sometimes a division will ask us for more information before it will definitely accept or reject a project. The development research group will supply that information most of the time."

The director of laboratory research, Peter Kronig, was described by one of the senior research managers as an example of AMC's policy of promoting for performance. Kronig's rise had been rapid. He had started as group leader only four years before being promoted to his present position.

Dr. Kronig divided his section into three groups: exploratory; scale-up; and analytical and characterization. (See Exhibit 2.) Discussing the problem of evaluation with the casewriter Dr. Kronig said: "Part of the problem of research evaluation is that in order to get early feedback we are trying to establish early communication between the scientists and the people who will evaluate their work in terms of application, marketing, and market testing. But I am not completely sure that market considerations should be particularly valid at this time. For example, there was a project that we really kept going by faith, and would have had killed if earlier evaluation had been done at that time. This project is now very successful, so I think we have to recognize that there may be some lost opportunities if we do project evaluation very early. Ideally, we should first decide on the areas that we want to do research in, and then go to the scientists and say, 'What can you give me in that area?' I think it is more effective if you make a suggestion to the research people within a limited framework than if you just let them go with a free hand."

CASEWRITER: Do most of the projects now initiate at the bench?

KRONIG: Yes. This reflects a serious deficiency in the whole of AMC. The research does not exist as a planned function. We would like to fill this void. There is a corporate planning office in Pittsburgh whose main function is to coordinate the divisional plans, but they do not establish corporate objectives. As a result we see ourselves in a logical position for this function. We are in the natural position to feel the needs of the divisions and at the same time have the corporate point of view.

CASEWRITER: How would you decide on an area of interest for the company?

KRONIG: We haven't come to any resolution of how to decide on an area.

CASEWRITER: How much true basic research is being done at CRL now?

Kronig: In the past two or three years, we have begun to do some basic research. I think the greatest part of it has been in Berger's group. He has been able to do work without any economic justification. Just doing the fundamental research on new alloys. Our plan is to increase our effort in fundamental research. I have been trying to push for this.

Casewriter: How would you estimate your total percent of economically unjustified work that you are doing at CRL?

Kronig: Well, very roughly I would say that 5 percent of the total budget could be spent on work that was not economically justified. I am not sure that I want to be quoted on this, but I have the feeling that this is just about right. This doesn't mean that only 5 percent of the work is basic or fundamental research. That figure would be much larger—about 10 to 15 percent. But some of this work can be justified economically. We don't really feel that perhaps they can knock off early on a Friday afternoon and do their own work, and I don't think we really want this.

We prefer to have the qualified man do this fundamental research and let the applied people do the application work. Besides increasing our fundamental capability we have to increase our effort in applications work. We need to convince the divisions that the work we are doing is of interest to them. And in order to do that we have to convince the divisional marketing people. So we need new applications people to carry forward the findings of the fundamental people so that more detailed data can be given to the marketing people in the division. We therefore feel that we have to tie any fundamental research effort to an applications effort.

So actually the scope of the CRL must be expanded in all directions. All of this has to be tied together with the economic analysis section. And this is true because the corporation does not have a strong enough planning function. If the corporation would exercise a stronger planning function, some of this could be done from above. Then the corporate planning function could say "We have to go into this area," and tell us to investigate the possibility of working in that area. But since the corporation doesn't fulfill this function, we have to do it here, and the economic analysis section definitely is a key group. We all want to beef it up. We are looking right now for someone to head it up.

Dr. Berger was described to the casewriter as the senior scientist who was considered the most creative scientist at AMC. Dr. Berger's group was engaged in most of the fundamental work being done at the CRL. The group was described by one manager as "the lab where most of our original ideas come from." Dr. Berger had been a research associate at an Ivy League school and had been hired directly into his present job two years ago. "I chose CRL because it was the least product-oriented outfit and because they made me the best offer in terms of money and freedom,"

he said. When asked by the casewriter to describe project selection within his group, Dr. Berger said: "An idea occurs to you and you select between ideas. Ideas occur to me because I think in terms of models; that is, certain qualities are desirable in the material. I then postulate a structure which should exhibit them. The next question asked is 'How can we get this structure?' At this point, you have a product and a process to reach it, *and* a large element of doubt because these are just paper structures and paper processes. Having gone this far, we then select from all our ideas on the basis of the potential end product, of the expected gain, and of the probability of success. Now this sounds very good; I guess everybody operates this way as far as he is able to. I think success depends on how carefully you determine the routes and the goals."

CASEWRITER: Do economic considerations come into the picture at this time?

BERGER: I have to answer that "yes and no." I probably take economics into account more than I should. When we are working on a new material and I am involved in the scientific aspects, I would not consider economics at that time. But when the material characteristics are reasonably defined, I start to think in terms of the economics; that is, in terms of the routes for making this product economically. Basically, what I've done is to demonstrate that there is a feasible route for making these alloys. These alloys were thought to be uneconomical because the ingredients going into them were considered too expensive. I've reexamined the reasons for the expense behind the ingredients and in certain cases have come up with alternate routes that make these products feasible. Also, I am working on a long-term project on semiconductors, and in this price is completely meaningless because performance is the main criterion. The materials in use now are all extremely expensive. We just go ahead and make what we want to make, what we feel is reasonable, and don't worry about the economics.

We don't know where we are heading and we don't worry about it at this point. I say let them worry about the economics five years from now, if we have something commercial then.

CASEWRITER: Do you consider the market consequences and end uses, as well as the economic routes?

BERGER: Yes, whenever we can, whenever it can be systematized. If I suggest a material, I have thought about end uses. For instance, I was pushed into the corrosion resistant business by upper management. We had to use a commercial material which cost $60 a pound as one of our basic elements. Well, the divisions felt that a cost of $60 a pound would make it impossible to compete with present products, and they felt there was no way around existing patents that protected this material. So I went ahead and postulated a new chemistry with new reactions and with new patent possibilities, and we are now testing this. We don't know how good

it will be but I have a feeling it will work. And one thing is clear—the material will certainly be cheaper because I used raw materials and routes that are cheaper.

CASEWRITER: Who usually takes these projects over?

BERGER: It depends on the projects. For instance, take the SM alloy. A few months ago Guthrie was assigned to one phase of the project; he assigned just one person to it and nothing was accomplished. It was a difficult problem. Finally, now that I'm almost finished, Capa was requested to work on a different phase and he is working on that very effectively, trying to get a larger quantity and a better quality of a particular alloy so we can do a characterization on this material. If Capa makes enough, and if it turns out well, he will see how the process can be developed. Other groups will also be working on this project concurrently. Then the micropilot plant will come into it and there will be extensive testing. At this point, the divisions would come in.

I would say that, in the case of the SM alloy, the interest has been developed in the upper management, rather than at my level, and management has been selling the people at my level. The men at the bench all have their own interests so they are rather reluctant to drop these to start on a new project. This reluctance on the part of people on my level to take over a project is of course felt by me and by my group, and it affects the way we work.

There are many factors that I don't like to take into consideration but that I have to in working for this company. This means that in many cases I choose to work in areas that may not be the most scientific ones but that seem to be the ones that interest the company the most. It may not be what they really want to do but that is what has happened. I can't operate in a vacuum; I have to be part of a cooperating group. When I stop being in a vacuum, by suggesting work that is essentially short-range and includes me in a group working here, then I am told that I should be concentrating on long-range work; that it is long-range work in which the company is interested in getting from me.

I don't know if my dilemma is clear to you. If I do what they want me to do, I will be more isolated than I have been in the past. Furthermore, the company does not reward for this kind of work. For instance, I tried to push them into the direction I think the company should be going, as in the field of semimetals, because I feel it can be extremely important for the company to get into this field. We're in the phase now where very fundamental stuff is being done in it. If the company does some work in it now, five years from now it may have the patents sewed up in at least one phase of the field. If they wait, all the patents will be sewed up by other people and all they'll have left will be the leavings.

But I had a very hard time selling management on this. Charles Thompson is quite conservative in his approach to these things. Management says it would like to increase the effort in fundamental research, but

the way the lab is organized makes it very hard. The lab is split into functional divisions. This is very efficient for development and testing, but when you have to understand all of the phases of research involved, this division is a hindrance. It would be very difficult, for instance, for a man who is involved in the early phases of research to go down and use one of the characterization machines himself. There would be all kinds of jealousies cropping up that would make it impossible. In some other companies, fundamental labs are set up so that all of the machines and equipment are there, and a man who wants to use them would get instructions on how to run measurements, and then would run it himself. He would take materials through the testing phase and slowly develop it —do some of the applications work and so on, if he felt like doing it. But the way we're set up here, anyone who is in the physical testing group is under pressure to do service work and it is difficult to get them to adopt a nonroutine attitude.

Presently, there are half a dozen people involved in all the phases of characterization. Each one is a specialist. What I would like is a more efficient system in terms of research rather than service. This system that we want may not be efficient to use in terms of a production line ramming out results, as they do in the physical testing group, but it would be efficient for us because very often the tests do not measure what you, naively, think they will measure. To get an observation of unexpected results requires close observation and familiarity with the material.

The situation in the physical testing group, it's very bad. The group is so large, there are four levels from the guy who does the work to Van der Bunt, the supervisor, and that's too many. So the man who does the work has to send his reports up through three levels before it gets to Van der Bunt. This is true for any data that needs interpretation.

Then Van der Bunt will write all the reports that he gets for a general CRL report. I think this is because the group had been organized as a service group historically, and they still think like a service group. I was eventually getting the reports when all the work was done, and the poor guy under me never got the information.

CASEWRITER: Couldn't he pick up the phone directly?

BERGER: Of course, but my man is really a metals physicist and doesn't have the background or the interest in the whole testing process. Part of my problem is to arouse interest in my man in that side of the problem. He should be getting the data faster and he should be learning what the relationships are between the metallurgy and the physical properties. It's really a question of basic attitudes because efficient service work means inefficient use of equipment, and the service group is quite jealous about their equipment.

CASEWRITER: Do you use the economic analysis section?

BERGER: I don't work the way that they do. I start with a dream. It's silly to do project analysis at that stage. How can they evaluate a dream?

There's just not enough data. They probably work all right with a lot of data, but our data is too premature. If I'm told that the raw material is too expensive, I start asking what makes it so expensive and I find a way of making it cheaper. Economic analysis looks only at the present prices unless they are requested to do otherwise, and there is no way they can know what the prices of other materials could be. There is no one down there with enough imagination. Actually, I do my own analyses at a very crude level and if it seems at all reasonable I go ahead. Any route I find will get much cheaper after a good development man works on it.

Another very important aspect in selection is that every lab has a certain atmosphere and this atmosphere pervades through the ranks. The atmosphere here does not favor research, but products; that is, development work. I even get it from my own group. There was a man who was working on the SM alloy six months ago. He asked me why it was that no one in the whole lab was interested in what we were doing except me. And there *was* no one who seemed interested. Of course, now they have picked it up and are interested. But this atmosphere exists and this is one of the factors I have to consider when I select a project: "How much interest can I generate with this project?" But I don't want to do research the way I feel they want me to do it, that is, to synthesize one alloy after another and to shoot them over to the testing lab for tests. I want to solve a problem from a theoretical aspect that refers back to a basic understanding of the structure.

The casewriter secured further data on the selection and evaluation process from Warren Law, one of the two men working in the patent office at the CRL.

LAW: There are two groups that provide the most patents—the forward-looking group in the lab and the forward-looking group in development and applications research. There is a feeling that many people could turn in more information for patents but that they don't do this because there is not enough time for them to develop a patentable case. When management considers a case sufficiently important, we get enough information and we spend considerable time with the men in developing the applications. But this is only when management considers this case to be sufficiently important, and also because the man was permitted to spend enough time on this process or product to develop a case. In other instances, where there is not enough time for the man at the bench to develop a case, we are not in a position to see what he did, so no patents come out.

We don't like to submit a case to headquarters unless we have tested it out as much as we can. We can't do this unless the man has enough time to spend on the project, and the man doesn't have enough time to spend on the project unless management considers the project important. So it turns

out that in most instances only projects that are considered important by management get patented. It runs in cycles, according to the urgency of the areas.

We also are limited on personnel up here, so we have to spend time in the area that the company wants. In many cases it would require very little extra work on the part of the engineer to give us more complete information and to present a patentable case. But as a rule, information is not submitted to us; it is obtained by us. There is some feeling that some of the fellows are getting the larger share of the patents because they are working in the areas the company is interested in.

Barney Ross was a market analyst in the economic analysis section of the Central Research Laboratory. Ross was a young man of about 25, dynamic, and articulate. He was well thought of by management, had a degree in mechanical engineering from M.I.T., and was currently working on his Ph.D. in economics.

Ross spoke about project evaluation as follows:

ROSS: The company is just now beginning to develop a formal evaluation process at the research level. Previously the only screening process that took place was to list those projects which were within the company's general field of interest. It took time to realize the need for screening, for research cannot be measured for effectiveness as easily as other functions can.

CASEWRITER: Barney, how would you try to measure research effectively?

ROSS: One possibility, I think, is to consider product mortality. Now if you draw a chart, and put "time" on the *X* axis and "number of projects" on the *Y* axis, you'll get a fairly normal curve and you'll see that the greatest mortality in number of projects is very early. I think you can plot this kind of a curve and compare it with yourself and other parts of your development history. Then you can try to raise the number of accepted projects that go to commercialization per number of original ideas and make some sort of improvement in that direction.

Perhaps the company should give the personnel a framework around which to work so that a self-screening could take place. Thus, not so many projects which would have little chance of being accepted would be proposed. I don't want to reduce the initial area, but I would like to make them more pertinent to the goals of the company. I think the goals of the company should be articulated. I think we need more interaction between research and the marketing people, between production and marketing people, and so forth.

I started to work for AMC at the beginning of the year and I am doing what I was hired for—to work on the economic aspects of new product ventures. The idea behind this is that we want to take a project at any

stage and be able to evaluate it. Now the way we do this is varied, but we often go to the market for estimates of dollar volume. More extensive product data is required to produce more accurate evaluations.

We have here at CRL a number of fundamental research projects which we call short-term projects. These have no allocated budgets and are quite informal. When a project starts to look good, a budget is allocated. More applications work is done and at this point there is a need for further market surveys. Later, at the final phases of the project when there is quite a heavy investment in development, an extensive market survey is done. We had one example. An idea got approved, applications work got done, and several potential products resulted. We have already done the field testing for this and expect to do a thorough marketing survey after the divisions will have picked it up.

The idea of doing market research at every stage is very new for Central Research Lab. Divisions often do their own studies and often we work together with them. The market survey is only a screening stage for the research. We are at the stage where we guide research by means of market considerations, but we have not yet reached the position where we can tell research what they should be doing for the future. Almost, but not yet.

The way we do the studies differs from case to case. Often if I don't have enough market facts I sometimes do a minimal profitability study; that is, given the minimum profitability required by the company, how much do we have to sell in order to make that minimum return that the company requires? I have developed a probability model for market penetration. And I am currently working on preparing some other models that we can use for market planning.

CASEWRITER: Do you have trouble selling this idea to a man at the bench?

ROSS: Not really. Surprisingly not. We try to sell a project manager and research management, and they have, surprisingly, been quite willing to accept some of these tools. Our main contact is with the project manager. And by and large they don't disagree with the recommendations that I come up with.

The casewriter interviewed Dr. Henri Le Sieur, the associate director of the CRL, to get historical background on the CRL. Dr. Le Sieur had been with the American Metals Company since the mid-thirties. He was director of the CRL in 1958, then a serious illness hospitalized him in 1960. During his convalescence he was named associate director and was expected to act in a consulting capacity. This position was described by some executives and scientists as "the top of the scientific ladder." To this Dr. Le Sieur commented: "I don't know if I am at the top of the scientific ladder, but I don't think they can hold out my position as a goal to the scientists. My position is due to my experience in running several success-

ful projects. Management needs someone here who is an old-timer and knows the ins and outs of dealing with the divisions. I don't know what I am, but I know that I am freer than Charles Tompson, the director of the CRL; that I can tell people what I think without being too careful. Also, Charles has a lot more red tape to deal with than I do.

As an example, American wanted to build a multimillion electrochemical complex. I took a look at this for about three months, without being asked, and found that it was not very sound. I felt free to tell them because I was pretty independent. As it turned out, they would have made a big mistake, but my telling them hurt the feelings of a lot of people. I don't know if Charles Thompson would have had the independence to tell them. You see, one of the problems is that we have to sell the divisions and more or less play politics with the divisions.

My point is that this lab should not be used to solve the divisions' problems. The reason we are doing this defensive research is because Stone, who is vice president for R and D of the corporation, can control this lab, but he cannot control the research being done in the divisions because the divisions control their own research. So, when Dr. Stone comes here, it is often to discuss the work we are doing to solve today's problems for a particular division. The CRL is, in effect, being used by the corporate headquarters to put out divisional brush fires."

Dr. Le Sieur said that since being named associate director he had been increasingly concerned with long-range planning and the long-run implications of present decisions. Dr. Le Sieur held the opinion that the present organizational structure of the lab influenced the selection and direction of research projects. He said: "Look at the number of people working in the physical testing group [25]. Even if you are not a physicist you will be tempted to do solid state research because the physical testing group can characterize a material for you. Dr. Stone was in here yesterday and asked me to do some more work on Alloy F. I said, 'I don't have the people to do the work. All the people that I have are working on the Alloy B.' He said to me, 'Well, if you don't have the people, hire them.'

But I don't think this is the right approach. We should be hiring for personnel needs for the next three or five years, not just to fill an immediate need. Because if we hire more physicists now, it will even further commit the company to be in the field of rare alloys.

When I hired the men of the PTG to take care of that day's immediate needs, I did not suspect what would happen; I did not look into the future. And in looking back I think I made a mistake. These people are here today and are one of the company's strengths. And yet they are committing us more and more heavily to rare alloys although this may not be the best area for the company. I don't believe the long-range solution is in letting the research people work for the marketing people, because the marketing people are product-minded and can not think about the future."

Chapter 23 (Continued)

*AMERICAN METALS COMPANY (B)**

Charles Thompson, the director of the Central Research Lab (CRL) of the American Metals Company described the process of transferring projects from the laboratory to the divisions in this way:

"We don't have a rigid system of transferring. We try to sell a project to divisions as early as possible. We feel that ideally the divisions should do the development and applications work, but we know that realistically we have to do some applications and development in order to make the projects more attractive to the divisions.

Yet each project is unique and is transferred in different ways, at different speeds, and with different successes."

CRL management was interested in an increased effort in applications work within the laboratory so that projects could be transferred out to the divisions earlier. Increased work in the applications area was also seen as a way of providing management with greater control over projects.

The casewriter attended two meetings where applications were discussed. The first was attended by Emil Bradford, assistant director of the CRL, Peter Kronig, the lab research director, and Herbert Danton the development research director. The purpose of this meeting was to review applications work done at the CRL and formulate a general proposal to meet future needs. Emil Bradford started the meeting by stating that applications work in the CRL was presently being done in both the laboratory research and development research groups. He asked both men how they decided where applications research was going to be done.

Kronig: It's been done in the past on a hit-and-miss basis. I can only give you isolated instances. For instance, on the Alloy E process it seemed that it would be a good idea to coat Alloy E with some materials and try the applications. Well, there was nowhere in the lab that it could be done, so we set up a jerry rig ourselves in our fundamental group. We were forced to do it ourselves—the same thing happened when we wanted to make some material. There was nowhere that we could go to do it, so we did it ourselves. At that time the development group was busy doing just the Gamma project.

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Bradford: Peter, was your group the most qualified to do this?

Kronig: No, but this process is a tricky system. We needed careful control and we did it ourselves because there was no one else who knew how to do it. However, with a group that was scientifically trained and equipped, it would have been easier and better if they had done it.

Danton: The Alloy E process is a special case. In most cases the end product would be a granular powder. In that case we could not have done the applications research quite that way. It's not often that we go to the forming but in the Alloy E, it was necessary because we needed to find out something about end-use properties and in this case only a physicist could have done the applications work. In other words, we needed special scientific skills to do applications work at an early phase.

Bradford: At what point did the development group get into this for making large batches?

Kronig: At about the same time.

Bradford: So the two groups worked on the same project at the same time?

Kronig: Yes, but the development group was called in to make larger quantities.

Bradford: Well, let's go to a more routine sort of project where the end product is a powder.

Kronig: All right, let's take as an example the SM alloy that we have made. We only have very small samples of this, and the physical testing group has done some work on it. It gives us an early evaluation picture. From this we have to decide which way to go.

Bradford: Who decides?

Kronig: The project manager.

Bradford: Well, does he decide this alone?

Kronig: Usually, yes.

Bradford: Does anyone else make a decision at this time? Do the divisions come in at this time?

Kronig: The divisions are not yet interested at this point. Actually, in this case the decision was made by myself and the project manager.

Bradford: Why do you think that the divisions did not get interested?

Kronig: Well, some were. C division, for instance, brought itself into the picture informally by means of an informal contact. They thought they might use this material as a coating. We thought that they could do the applications work on this material as they have the capability to do this kind of thing. We realized that they needed larger amounts of the alloy, and Berger's group was not really set up for that. They have to start a project and then, at a certain phase, they lose interest. Capa, however, was interested because it tied in with some of his earlier studies and with some of his interests, so Capa took over the work on that project.

Bradford: Is there a chance that Bob [Capa] is not as interested as he could be because the project did not start in his group?

KRONIG: This might be, but I think this is fundamental enough to interest Bob.

BRADFORD: What is the future of the project now? At what stage do you envision a transfer to the divisions?

KRONIG: There should be more characterization work done first. There is some conflict of opinion among the workers but it seems to me we need a pound of the material to do a decent characterization job, and this means that development will have to make a pound for us first.

BRADFORD: So at what point should Herbert's group [development research] get involved?

KRONIG: When we get a good pound of the stuff, we can do the characterization work, and after that's done we'll need about 100 pounds. At that point it seems to me that Herbert should come in.

BRADFORD: Framing the question in a more general way, where in the organization should applications be?

KRONIG: It seems to me that we can do applications very early in the game because there's beginning to be a theoretical approach to determining characteristics on the basis of building blocks. So it seems to me the applications group should be an adjunct to the physical testing group.

BRADFORD: Do you have the nucleus for such a group?

KRONIG: No, but I'd like such a group.

BRADFORD: Herbert, do you have a group down there who can take over applications work, a group that will take over work that comes down from the lab?

DANTON: We have no expert in this field; they are all flexible guys and will do the job, but they will have to do it the long way around. We can't get a real specialized group because it will duplicate a lot of the work that is being done in B division and this gets very sticky. We always come back to the basic problem that CRL is not a manufacturing outfit and that we have to "sell" the divisions. Until this policy is changed, we can't get into this specialized end of applications too far.

BRADFORD: So it would appear, from this, that the CRL should have a group that is a Jack-of-all-trades rather than specialists, since we have the specialists in the divisions. What are your comments on that?

DANTON: The problem is that the divisions should pick up on the applications research when it gets to a batch scale.

KRONIG: We need a large batch sometimes to do characterization work, and early applications to sell a project to the divisions.

BRADFORD: All right. The two of you get together and make a recommendation in writing by the end of the week.

DANTON: I really would like a large applications group, but it's going to be rather hard to justify, I think.

KRONIG: I think that if we could get a large economic analysis group we could justify a larger applications group—so that we give a division, in a sense, a much more complete package.

BRADFORD: On this question, why do you think we have trouble selling to the divisions?

KRONIG: It's a combination of factors.

BRADFORD: I think we need a more thorough look at the competitive markets for new products so that the market can be pinpointed. We also need sometimes to go a little further into the manufacturing process. The question is, for instance, could we have sold aluminum siding to any one of our divisions without going into the development and manufacturing process in great detail?

KRONIG & DANTON: No, we couldn't have.

KRONIG: What I want to do is get a long-range group to study new techniques of manufacturing for future products.

BRADFORD: This is good; this is very good. Put this into your report for the applications group. Personally, I feel that we should have an applications group both in the lab and a sister group in development. But I don't know whether I'll be able to sell this. Let me sum up. We'd need not only early applications work to develop new manufacturing techniques, but also close coordination with divisions. Submit your recommendations in writing so that we can follow up on this.

The second meeting on applications attended by the casewriter was held in Peter Kronig's office. Kronig had called the meeting to discuss with Walter Van der Bunt (supervisor of the physical testing group) and Bob Steinpilz (a physicist who was a group leader under Van der Bunt) the possibility of establishing an application group under Van der Bunt. It was thought that such a group could handle all the applications for the laboratory research section of the CRL.

Bob Steinpilz had been described to the casewriter as especially interested in applications, having personally attempted to interest some industries in new materials developed at the CRL.

The meeting started with Steinpilz reporting on his trip to discuss possible applications for Gamma material for flexographic printing. At this point Dr. Le Sieur came in and, seeing Steinpilz, said, "Ah, just the man I want to see. Dr. Stone [corporate vice president, research] is here. Did you draw the Alloy F? I went down to your group. Nothing had been done while you were away.

Steinpilz was surprised that nothing had been done and said that he would see about it after the meeting.

LE SIEUR TO VAN DER BUNT: When can I get this stuff tested?

STEINPILZ: It has to be conditioned first. The conditioning takes time. I don't think we can do the testing until Tuesday.

LE SIEUR: Well, in my current report I said that the testing would be done by now. Now if I make another report and say that the testing will be done Wednesday, and it's not done on Wednesday, how long does this go on?

STEINPILZ: Just what is it you are trying to get at? The patent position on this? We can give you numbers now, but they would be meaningless; we have to take time to condition the material first.

LE SIEUR: We're just trying to find if this alloy has any properties that have an advantage over the other.

STEINPILZ: I can make a prediction just now, and that is *no*. That's why we should establish what you are trying to find out.

LE SIEUR (with some emotion): Well, you can always say you don't think a thing will work but you still have to do the test with an open mind.

VAN DER BUNT: I don't know why we are doing this testing. I don't have too much hope for this project. (LE SIEUR exits)

PETER KRONIG: Yesterday we had a meeting, Herbert Danton, Emil Bradford, and myself. Emil wants our recommendations on how we are going to handle this applications group. Briefly, Herbert said that in order to do true applications work we need a large applications group. He also said he can't justify such a group at this time. Part of our problem is what do we mean by applications?

I see applications as three possible things. First, given conventional techniques and material, to make a new alloy; the second, using an alloy that is known and unconventional techniques, to make some new end-use; third, given an alloy and a new technique, to work out a new application.

Each of these applications involves a totally different concept and involves different people, and I don't think we can tell at this point what it is that we can do best. My thought was that we should have a group of four or five people located here in the lab group and working closely with the testing group and with the economic analysis section. This group should work on new fabrication methods and on new materials. Now can you give me your reactions as to what we can do with four or five people, and do you have any four or five people in mind that we can pull out of the divisions or that you know from outside that we can hire? Just how much can we do with four or five people?

STEINPILIZ: Is all of this before going to the divisions?

KRONIG: Yes, or in certain cases we could work with the divisions on a very close feedback.

STEINPILZ: My question is that we have to recognize that we have to meet different levels of sophistication on this problem at different stages.

KRONIG: It's really finished for us when the division takes over. It depends when the division takes over, on the division involved, and on the product. We have to give the division enough information to sell them.

STEINPILZ: But how far do you want to take this? This is what I don't understand—and how fast do you want it? I'm just trying to get an idea of how these four or five people are going to work.

KRONIG: We have to sell four or five first, then if this works we can get a higher number of people.

STEINPILZ: It's easy to bring a project along to this phase where people get interested, but to get it refined takes a good deal longer. The question is and this is brought out by the printing industry, how far do we want to go on new processes? For instance, can we ask for a new alloy because we feel there is a potential market?

VAN DER BUNT: I don't know. Can we at this time postulate a new structure from the needed characteristics?

KRONIG: We are getting to that stage, in understanding structure-property relationships. I told Bradford that I would give him a recommendation by the end of the week. I need information on equipment, on people, and also on who will lead this group. Walter, will you head it up?

VAN DER BUNT: I don't see how this will work.

KRONIG: Well, for instance, you would test a lot of materials and then you would go back to the lab and you would say "We need this and this modification of the material so that it has these properties so that we can sell it."

VAN DER BUNT: But we wouldn't make the alloy.

KRONIG: No. But it will all be in the lab research family so it won't be very hard to get the alloy made.

VAN DER BUNT: Well, we'll need a metallurgist anyway.

KRONIG: Of course you'll get a metallurgist. Now, you should work very closely with the economic analysis people. They have to be in a position to see the whole of the picture, and they are in a position to have perspective on the company.

STEINPILZ: At what point do you go to economic analysis? In the beginning, when you first get the idea, or after, when you've worked a lot on it and you can say "Here, this is what this product can do?"

KRONIG: I see no reason why the economic analysis section cannot be in on the project at the very beginning.

STEINPILZ: But I'm afraid they cut out a project at its first sign of failure.

KRONIG: Your group certainly can initiate projects, but economic analysis should be informed. They're getting stronger about the work they are doing and they will be getting even stronger in the future.

VAN DER BUNT: Do you think this group has to prove itself in the beginning with four or five?

KRONIG: Yes. On a thing like this it's very much the better approach to go slowly. If we go too fast, we can make an error and take on too much work—and have it chopped off. I think if we go slowly, and prove ourselves as we go along, we can grow.

VAN DER BUNT: What kind of application do you want to do? There are two approaches to this: you can just act as a testing group or you can

initiate work by having a close contact with the lab people. Let me ask again how far do you want to go? If we have to get over to our customers a change in the historical specifications of a material, we need contact with far-sighted groups in the companies, so that we need a marketing man. We have to have this kind of contact to get the company to change its historical dependence on specifications for materials. So the question is: how far do we have to carry an application before we can sell it to the divisions and to the outside customers? And this involves, essentially, what kind of a service are we going to perform? We have to know this before we start hiring people.

KRONIG: So where are we on this project?

VAN DER BUNT: I don't know. Where are we?

KRONIG: Steinpilz, why don't you get the estimates on the machines? We'll have two technicians, a metallurgist, an engineer, an engineering mechanics man, and a supervisor.

After the meeting the casewriter asked Peter Kronig who would head up the applications group. He said "Steinpilz will. He's got the background for this kind of group." Peter Kronig was interrupted by the entrance of Robert Capa, one of the senior scientists under Kronig, whom the casewriter had been introduced to at a staff meeting.

Bob Capa had been described to the casewriter as the most dynamic man in the CRL, a scientist who had ability in both fundamental and development work and who had great potential for promotion.

CAPA (looking at the casewriter with emotion): He wants to know how research ideas come about? Boy, I'll tell you. We had a series of mistakes last week and then I had a brainstorm. It's not going to make money for AMC but it sure is going to help science. We are now testing all materials we have to see how good a test it is, and it looks great. I can get at least two publications out of this. Boy! (Explains process to Peter Kronig.)

KRONIG: Well, before we publish, can we patent?

CAPA: Yes, we can patent. I've seen this done before. We can patent a process.

KRONIG: Yes, but can we enforce it?

CAPA: No, we can't enforce it, but we can get our name on the patent.

KRONIG: How can we get something out of this for the company? That's what I keep thinking.

CAPA: Well, the scientific reputation of the company will help you in hiring new people. We can even get more than two publications; we can get as many publications as we are willing to stretch out. Each time we test a new material we talk about it, and the beauty of it is it took only one week for one man.

Kronig: Well, great ideas sometimes come just that way. What have you done so far? Did you write up the idea before you started?

Capa: Yes, it's all written up.

Kronig: Good. But I'm still interested in making some money out of this for the company.

Capa: Boy, if I was a businessman I could make a lot of money out of this.

Kronig: What would you do?

Capa: Well, I'd have the patent, and then I would make some $50 machine and go out and sell it.

Kronig: Well, that's an idea. (At this point all go to the lab and look at the discovery. Capa is very excited and keeps saying "Boy, isn't this great!")

Kronig (half jokingly): You see, Bob, you wouldn't have developed this if you had been doing fundamental research.

Capa (heatedly): O yeah? I sure as hell would. (Pause) You know, when we get applicants I really get very ashamed. They ask me what fundamental work I've been doing, and I say "Oh, some problems in corrosion of alloys," and I hang my head. Maybe I should change my name. What do you think, Peter?

Later, when the casewriter and Bob Capa were having lunch, the latter explained his remark about changing his name.

Capa: The trouble is that if you get the reputation of being able to take a project all the way through, they start to give you all of the other people's discoveries to take through, and if you do that it's sort of the end of you because when you do it for someone else you really wind up by being a development engineer, and you lose touch with exploratory work and with the theoretical approach. Then the title of senior scientist is really a fake.

Now I don't mind doing it once in a while for other groups, if it's for the good of the company, but I want to keep working on my own things too. I can not be like some others. The way they do exploratory work is by refusing assignments if someone asks them for help. If someone asks me for help, I will give it to them; I have to, I cannot refuse it. If everyone here would act the way that they do, there would be no CRL at all. The company would take a look and say, "What are these people doing? They're refusing to help us."

Casewriter: Then the way you see it is that by accepting an assignment in development work you are letting others continue with exploratory work?

Capa: Yes, that's exactly right. Being short of supervisors, they had to pick one of us to do the development work, and they picked me because I could do it. (Pause)

One of my men was assigned by me to develop Berger's work. That's

all right; I don't mind—it's for the good of the company. But then Berger comes in here, and without seeing me goes to my man and starts telling him what he should be doing. Last week, Berger came in and said to my man, "Gee, why did you do that? I could have predicted that this reaction would have come out that way." Well this discourages my man. And I don't like that because I have gotten my boys to believe that they are the best in the laboratory.

Now the reason I took this job from Berger's group was that I was told that Berger's group could not carry the research past the initial stage. So he feeds the tentative data to us and we take it from there. I have asked Berger several times to give us all the information that he had in a written report, but he has not been doing that. He's been coming into my lab and poking his nose into what we are doing. Of course, he is interested in his baby; I understand that, but he should not just wander in and influence my people by changing my orders. He should come to see me first. I don't go wandering into his lab and talk to or criticize his people. And my people don't like this either.

CASEWRITER: Have you tried talking with Berger about this?

CAPA: No, I would only lose my temper. I dont want to hurt anybody's feelings and I think if I talked to him I would lose my temper. You know, I asked Peter Kronig to talk to Berger. Kronig has worked under Berger in school. He also was in his group as a group leader, so perhaps he can talk to Berger about this. But I think it will be hard for him.

I put my best man on this project: this is what I resent. Each time my man does something good Berger goes to the man and tells him, "I could have predicted that." My man objects, of course, but he can't tell Berger because he is in a senior position and some day may be his boss. But instead of telling my man what a good job he did and how much he appreciates it, he tells him, "I could have predicted that."

Now that's no way to deal with people. I'm mad because he downgraded my man's work. If Berger could have predicted that reaction or approach to the problem, he should have put it on paper for us to see. Also this business of walking in like this and attempting to influence my men lowers my prestige with my group. (Pause)

But you got to say one thing about Berger, he sure has my admiration as a scientist. He is extremely well-read, encyclopedic almost; he knows more than anyone in the building. He knows so much that he can go to a meeting and talk complete nonsense and no one will know the difference. That's true. At one time, there was one small area which I had some knowledge in and in which I was able to ask a question to stop him. But most of the time he will talk through a meeting and nobody will know what he has said.

Something else happened just last week. I made a research proposal several weeks ago and the same proposal was made a week later by Berger.

Kronig called me into the office and starts talking to me about how great Berger's proposal is. I said, "Well, if you look in your files you'll see that I made the same proposal a week earlier. So from that standpoint his proposal is mine and I have the priority." But Berger said that he would do the work anyway, even if I had the priority, because he had the equipment.

I said "All right, go ahead, do it." (Pause) But this is just an incident, really. (Pause) The way this man operates is really terrific; I could really learn something from him.

The casewriter next talked to Milton Berger.

CASEWRITER: Could you explain to me how the SM alloy got transferred from your group to Capa's?

BERGER: I didn't transfer the SM project. There was a meeting of Kronig, Thompson, and myself to discuss where the work should go after it left my lab. Our group had finished and we looked for a group to continue it. Capa's group was the freest and it was picked.

CASEWRITER: Aren't they supposed to be an exploratory group?

BERGER: Yes, they are supposed to be an exploratory group. Capa is very energetic and very thorough. He is very good at what he is interested in. The trouble is he is interested in rather standard stuff. So when the chips were down the company saw fit to ask Capa to do this because this is where he worked best.

CASEWRITER: Do you still have an interest in the project when it is in Capa's group? Do you communicate with them about the research results and the goals?

BERGER: You mean, does Capa come to me for information? The answer is *no*. Capa thinks he is quite capable of doing this research, but the man who is doing the lab work comes to me to get all the data. My man who was working on this has since been transferred. I was just as glad to lose him because I don't think he was too good in this field. I mean he was not too well trained and his work was kind of sloppy. So I don't want Capa's man to go to my man as I don't think the information he would get would be very reliable. I'd just as soon Capa's man came to me.

Chapter 23 (Continued)

AMERICAN METALS COMPANY (C)*

AFTER SPENDING a week at the CRL the casewriter decided to explore the possibility that both the project selection and evaluation and the transfer processes were strongly influenced by the relationships between the lab and service groups. Peter Kronig said the following about these relationships.

KRONIG: It's getting more difficult. When I came here it was very informal. There weren't so many people in the labs. The scientist would just take a sample that he wanted tested to the man doing the testing and just ask for the work. And he would get it back. Now it is getting worse. There are more and more people and more and more samples. And the project managers system . . . (Pause) We don't know just how this testing should be run.

We have tried to set up a system so that each project has a liaison man in the physical testing group who does the initial screening on what should be tested. We have more testing now because our efforts to get earlier evaluation requires more testing and because a lot of work comes in from the divisions. The physical testing group finds it easier to set up to run a standard test. When something comes in that is something of an oddball, there is a tendency to shunt it off and not to do it. So we set up a system of having a liaison man there, usually a group leader, and this should be something of an antidote. The coordinator should be in a position to direct the material to the proper channels so that it gets done. In Berger's case we set up a system so that he can get information fed back to him weekly.

Milton Berger reported to the casewriter that he had experienced some problems with his liaison man in the PTG, Jack Redmond. Berger then related an incident to the casewriter. He had wanted a wire drawn (to orient the molecules for an X-ray) by Carlos Landa, an X-ray crystallographer working in the analytical section.

BERGER: Redmond knew why I wanted to have the drawing and yet he deliberately miscommunicated to Van der Bunt. In fact, he told me as

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much when he saw me. He said "Well, I didn't tell Van the whole story."

For some reason he didn't want to do the work. I don't think there is any other explanation than that. When I told Van later what I wanted it for, he told me to give it to Landa. Landa was quite upset. He said that it takes him at least a month to get any work from that group and it was Redmond's responsibility to have the wires drawn. I think this whole thing could have been settled if this fellow Redmond wasn't so negative. It would have required about two hours' work for him and the service group. I told him what I wanted in detail and, as a Ph.D., he should certainly have understood.

Dr. Landa, an X-ray crystallographer, led one of the four groups under Dr. McDougal. He explained the drawing incident as follows:

LANDA: This is mostly a question of different people thinking different ways. You see, this is the value of the Ph.D. A man who has a Ph.D. is interested in the problem and is used to working under adverse conditions. He will think of new ways of attacking the problem and he understands the basic nature of the materials and the machines with which he deals. He gets "research minded." The others understand the machines with which they deal, but are afraid to change the normal settings. They are not really considering the basic meaning of the numbers they turn out. They get "technician minded."

You see (pointing to the X-ray machine), here is a machine which I fixed to be more flexible in its approaches to X-ray diffractometry. These little items that I put in here cost a total of $10. If these were bought as a standard item on the machine they would cost an extra $1,000. I am able to change the setting to work at conditions that are far away from the original design. This approach is very foreign to a "technician mind," who is constantly turning out the same kind of measurements on the same kind of form.

We had a sample that we wanted to draw into a wire so that we could get orientation of the molecules to get an X-ray picture. I could do this myself. In fact, that is what I did. If I can do it, then a man who has the proper machine must do it better than that. I didn't expect him to make a perfect wire, I just wanted him to make it better than what I had. But he doesn't think in these terms. This man feels that if he takes on a job it has to come out within certain specifications. The specifications are meaningless because they were not established for this new material. So, it is not a lack of interest in the problem but something more deep: a different kind of way of thinking.

When Landa was told by Redmond that it would take one month to draw the wire, Landa went back to Berger and told him he could not get the drawing done and that the work would be delayed. Berger went to

Kronig who called in Van der Bunt. After some discussion Van der Bunt assured Kronig that the wire would be drawn in two days. The conversation continues:

KRONIG: In general, do you want Berger's man to go directly to your man?

VAN DER BUNT: Yes. Ordinarily, Berger's man could come to me but I guess they heard that I was pretty busy right now, and so he went to Berger.

KRONIG: Well, do you want to open the flood gates? If Berger gets the chance he will send everything down here that he can send. You know that.

VAN DER BUNT: I know. But I don't want to be bothered passing on every little thing that comes through.

Talking with the casewriter later Dr. Van der Bunt said of the relations between his group and the lab group:

VAN DER BUNT: We get along better with some than with others. The best is Berger. We differ sometimes on the interpretation and the work that has to be done but I think his grasp of our approach makes him easiest to communicate to and I think that he understands our problems the best; so I would say that he is best to work with.

CASEWRITER: Berger is easy to work with?

VAN DER BUNT: Yes, he has a very broad understanding of our field. I will argue with him, but this argument is not friction. Just the other day he asked me to get his alloy drawn to a wire and I asked him to go to Landa and let Landa arrange for the drawing directly with my man. But this is argument not friction.

CASEWRITER: Why didn't Berger go to Redmond with his problem?

VAN DER BUNT: He did, but Redmond wanted to do a whole series of tests, to which I objected. When I met Berger in the hallway he told me what he wanted, and when it became clear I saw that what he wanted was justified. By using a liaison man who is in contact with each project we avoid unnecessary tests because the liaison man knows the background of the polymer and what parameters should be tested.

CASEWRITER: How did this misunderstanding occur?

VAN DER BUNT: Probably Berger didn't explain just what he wanted to Redmond. If the man had known what Berger wanted, I don't think this problem would have occurred.

The casewriter next interviewed Jack Redmond, the liaison man for Milton Berger. He asked Jack to explain his job as a liaison man.

REDMOND: The way I look at it, while information goes through regular channels each day, some information goes directly to Berger verbally.

CASEWRITER: Is this to speed up the information flow?

REDMOND: Yes. Many times we have had the intention to write up

information each week but this has not worked out because we have been too busy, so the weekly information is just verbal. Frequently we get into a discussion on what the data means. I actually don't regard myself primarily as a liaison man. I am more like someone who is doing the work. I am not too concerned with communications. Mostly I am concerned with doing the work. I have some communication with the people who ask for data. I see that communication takes me away from my work.

CASEWRITER: You feel that communicating makes you take time from your work?

REDMOND: Yes. I signed on as a scientist not as an administrator.

CASEWRITER: Let me see if I understand what you are saying. You are acting in a dual capacity of man at the bench and liaison man. But you are primarily a man at the bench. And you feel that the liaison activity takes you away from your primary interest.

REDMOND: Yes, that's right. (Pause) But maybe the previous statements are an excuse for something that I don't like to do.

CASEWRITER: You don't like to communicate?

REDMOND: Yes, without a doubt; it's a sort of a crutch. I guess I just don't like to act as an administrator, and I use the other as a sort of excuse.

CASEWRITER: How do you feel about being assigned as a liaison man?

REDMOND: I don't mind it at all; it's a position of responsibility.

CASEWRITER: Why do you think you got picked to be a liaison man?

REDMOND: Because I was doing the work. I am someone who gets the work done, and I am not truly a coordinator.

CASEWRITER: If Berger wants work done does he come directly to you?

REDMOND: Yes—unless it's a major request or it involves a change of procedure.

CASEWRITER: I wonder if you could clarify something for me. I don't understand your feeling about the liaison responsibility taking you away from the bench, and your feeling that you were glad to have the responsibility.

REDMOND: It's really like two faces of a coin. If you want to advance in the organization you have to accept responsibilities. (Pause)

CASEWRITER: So there are two possibilities for you: advancing or doing what you like.

REDMOND: No, not for me only; there are just two possibilities in general.

CASEWRITER: So you don't see this as a conflict for you?

REDMOND: Not necessarily. Let me give you a parable. Suppose you have to travel to another city. You could fly there or drive there. But whatever you do the travel itself is unpleasant. You have to bear with it in order to get there. So if you drive, even if it is unpleasant, you do the best you can: you drive safely.

What I'm pointing out is that there are two faces to every coin. It's just

something you have to do. It's not an odious task but I can think of better things I'd like to do.

CASEWRITER: Let me ask you to fill me in on your end of a specific incident that I have some data on. Can you explain to me how you felt about this matter of drawing a wire for Berger?

REDMOND: This is an example that I referred to earlier. It's a change of procedure as far as the testing group goes. The general impression was that it was outside the scope of the outlined procedures for characterizing these alloys.

CASEWRITER: I don't understand.

REDMOND: I guess this sounds terribly like the Army. I guess in a sense it is. If I wanted to do it, I would have to monopolize the time and the instrument of another group, which I don't want to do.

CASEWRITER: You felt that this request would make you monopolize the time of another group.

REDMOND: Yes, I didn't want to do it for political reasons.

CASEWRITER: Did you feel that the people at that end would react negatively to your request?

REDMOND: Yes, they probably would come upstairs and it would get kicked around. This is a new case, we hadn't a set procedure for handling these new materials. We were not certain of what parameters to use.

CASEWRITER: Did Berger explain to you what he wanted?

REDMOND: Oh yes, he explained it to me in detail and I knew exactly what he wanted.

CASEWRITER: But you did not feel that it should be done?

REDMOND: No; I did not have the authority to do it.

CASEWRITER: You would have to go to Van der Bunt.

REDMOND: Yes.

CASEWRITER: You mentioned political reasons for not requesting the work. Do you want to talk about what these were?

REDMOND: The man who runs that group is an "old hand." He doesn't take too kindly to changes in set procedures.

CASEWRITER: And you did not want to annoy him with your request?

REDMOND: They have a lot of work to do themselves. They get quite crowded and have tight schedules.

CASEWRITER: Do you work with them on other projects too?

REDMOND: Yes, frequently. I am involved in projects dealing with physical chemistry and they have the machines to do the testing.

CASEWRITER: And you used them on some of your own work?

REDMOND: Yes. You have to weigh in your own mind what the value of the work you are requesting them to do is.

CASEWRITER: So you rely on them for your own work and don't want to alienate them by asking for something out of the ordinary?

REDMOND: Yes. I don't want to pressure them with requests that are offbeat. I would be asking them for something they are really not equipped to handle.

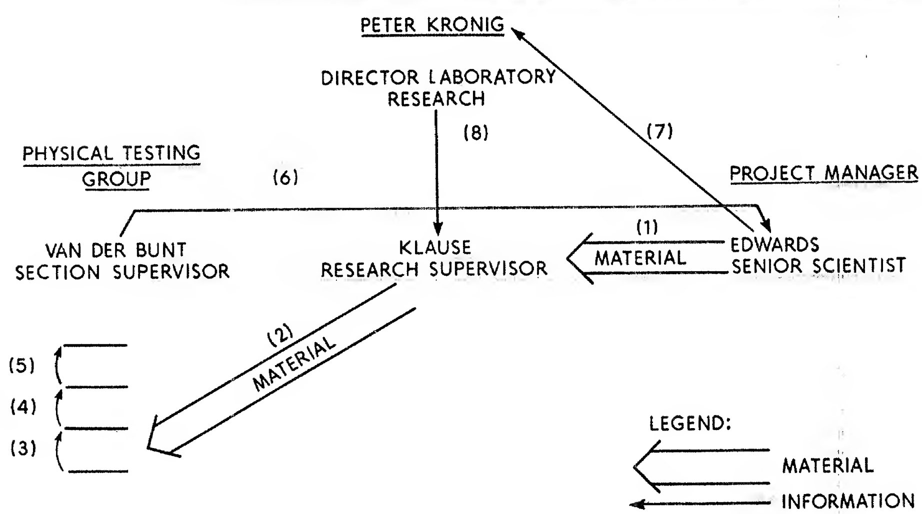
Casewriter: Can you tell me something about the work you are doing that you are most interested in?

Redmond: I guess it's work that comes from within this section. We analyze materials that seem interesting to us. We relate the structure and properties of these materials.

Dr. Heinrich Klause, a research supervisor, came from Germany six years ago. He was, in the words of one research manager, "A European-trained scientist with the European outlook. He likes to work alone on a project until it is fairly well complexed and doesn't like to talk about his ideas until there are established results."

Exhibit 1

INFORMATION AND MATERIAL FLOW DIAGRAM



Dr. Klause was presently working on the second phase of the Alloy B crash program. The first phase was in Dr. Edwards' group. The casewriter had been told that Dr. Klause had expected to be named project manager for the crash program and was very disappointed when Dr. Edwards got the position.

Dr. Edwards' group made materials to blend with Alloy B, and Dr. Klause blended these materials with Alloy B to find a material blend whose properties had the desired characteristics. The resulting blend was then sent down to the PTG for testing. The PTG fed the information back to Dr. Edwards (as project manager). Diagramming the information flow (Exhibit 1), Dr. Klause said it usually took four weeks for him to get feedback from the service groups.

Casewriter: What happens to the information that you generate after you get through?

Klause: When I get through, I write a report and, depending upon the

situation, makes recommendations for further studies or evaluation.

CASEWRITER: When you need service work, do you get all the information that you want?

KLAUSE: This depends. On simple analytical service we get exactly what we want, but most things beyond that, we feel, are sometimes not adequate. The PTG has developed the practice of declaring most of the necessary service work as "research effort" in which "professional people" have to be involved. This is even applied to cases where the particular tests are standard routine procedures and the effort of a trained technician would be completely adequate. Rather than reporting test results that constitute just straight numbers immediately back to the supplier of the sample, the group leader or supervisor of the PTG collects that data, inquires about additional background information (which is not necessary for the execution of the test), and writes a more or less formal report, including the work of the other groups involved.

CASEWRITER: Who gets these reports?

KLAUSE: The director of the lab, the director of research and development at AMC, and so on—it depends on the subject.

CASEWRITER: Could it happen that their report, with your data in it, is published first?

KLAUSE: Yes, this could happen and it *has* happened, and probably not everybody is very happy about this.

CASEWRITER: Do you feel a little hesitant about giving them information a second time?

KLAUSE: This is difficult to answer. I make the most effort to give them the information but I feel a little bit uneasy. I usually give the information and take the risk that nothing will happen that shouldn't happen.

CASEWRITER: What do you think is the cause of their publishing this way?

KLAUSE: Maybe they have the feeling that their standing may be judged according to the number of reports that they publish. The question really is, who gets the credit for the ideas and their realization.

CASEWRITER: The last time this happened was there a question about who got the credit?

KLAUSE: It depends on who was doing the reading of the report and how much that person was aware of what actually happened.

CASEWRITER: Suppose for a moment that the reader of that report was someone like Dr. Stone (corporate vice president, R & D). Who do you think he would give the credit to?

KLAUSE: It seems to me that he would just look to see who is the author of the report. The man in charge may say that this is not important. His attitude may be "I don't care where the information comes from as long as I get it." But it matters to me. I don't feel too happy when I ask for tests to get a few numbers and the man comes back to me sometime later to ask me about what I was doing and why I did it, and then I ask him

why does he want to know all of this, and he says I want to include it in my report. When I ask him "What did you measure? What were your results?" he says "You will see it in my report; I can't tell you because I haven't shown it to my supervisor yet."

CASEWRITER: Are you supposed to write a report of your own, in spite of the fact that they have written a report?

KLAUSE: Yes. But if there is already a report there is really no need. If the report is incomplete or contains wrong interpretations, it is sometimes not even worth the while because there may be hesitation to approve the publication of a report that may contradict some of the statements made in the earlier report.

With the system presently practiced by the physical testing group there is, of course, always the possibility that a project may be killed before it even started. There are many new materials that in all probability we would not have now if a system like this had been applied to those particular research projects.

Chapter 23 (Continued)

*AMERICAN METALS COMPANY (D)**

WHEN THE casewriter first discussed his field work with the research management at the Central Research Laboratory, he was asked to direct some of his attention to the so-called "dual ladder" of advancement within the laboratory and to the more general question of reward for scientific work. Dr. Le Sieur, then director of the CRL, had instituted a formal dual ladder in 1960. Management's current attitude toward the dual ladder was articulated to the casewriter by Charles Thompson, the present director of the CRL.

THOMPSON: The dual ladder was seen as a solution, or at least a partial solution, three years ago. Since that time we have had a second look and arrived at the conclusion that what is required is not an organization chart with some empty spaces. What we wish to achieve is the creation of individual positions to match as closely as possible the capability, desires, and objectives of the individuals involved. Thus, we are moving away from the rigid system and prefer to recognize positions which our senior research personnel have, in a sense, created for themselves. If we are successful in this approach, no rigid set of job descriptions and titles will completely define the nature and scope of advanced research positions. It will be necessary to create new titles to provide recognition for individuals in these positions.

CASEWRITER: This seems to be a ticklish problem.

THOMPSON: It is really an old problem that led to the dual ladder in the first place. We realize that to interest capable individuals in an industrial research career we must reward for research.

CASEWRITER: Do you feel that rewards for management and for science are fairly well defined?

THOMPSON: Considering administrative positions versus nonadministrative positions, it has been pointed out that the potential for reward is as great on the scientist side as on the manager side. Dr. Le Sieur is at the top of the scientific ladder. Some men in the organization may have made the decision that their goal with central research laboratory is to achieve the associate director position in the future.

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One difficulty which they may have with this objective is that there are no formal rungs on the dual ladder between the senior scientist and the associate director positions. We have come to drop the rigid organizational structure that is necessary for the formal dual ladder. We feel that it is not flexible enough to accommodate the differing personalities, objectives, and capabilities of advanced research personnel. To achieve the required degree of organizational flexibility we prefer to create positions to match the individual's capabilities and interests.

The feeling in the past was that the formal dual ladder was necessary to provide recognition and reward for scientific work. We would like to retain these benefits of the dual ladder system and eliminate the deficiencies of its rigid structure. We feel that salary is an important means for providing recognition and reward for outstanding research contributions. We would now like to remove the salary ceilings in each job category. Thus, if a man should continue on the scientific side he could expect to receive an increasing salary in this position and to maintain a salary consistent with those of individuals with similar capabilities in the line organization. A research scientist has the potential of receiving a salary as high as any associated with the line positions.

CASEWRITER: Dr. Thompson, you said that you had titles to give men for positions that they gave themselves. Could you elaborate on that?

THOMPSON: Yes, a man does create a position for himself in an indirect way. He doesn't create a position for himself mechanically—he doesn't change the title on his door—but through his capabilities and his interest he does create a position for himself in the organization. An individual may have a very conscious goal for himself within the company. We recognize this.

For instance, a man may want to function as an internal consultant for the company and he may be very effective in this position. He will function in this manner whenever he has the opportunity. Management will come to recognize that this man is performing a function that is not adequately described by his job description. If this is a function that we wish to retain, we then have the opportunity to create a new title and job description consistent with the nature of his function.

Of course there is a danger that the informal behavior of the individual is not desired by management. In a case such as this, we would hope that sufficient feedback would exist to allow the individual to modify his objectives. Thus, instead of forcing research personnel into the rigid titles and job descriptions of a formal dual ladder system we would develop titles and job descriptions consistent with the positions which men in nonadministrative functions have created for themselves.

CASEWRITER: You mentioned different abilities and goals of the senior scientists. Could you elaborate on this?

THOMPSON: Berger is very much the image of the true senior scientist. He is a brilliant man who originates many ideas. He originates new project

areas and tells the company: "This is an area you should be interested in." We know that he will not start a project in a field in which the company is not interested. Therefore we exercise a minimum of control over him and he feels he has a lot of freedom.

Bob Capa is a real go-getter. He has tremendous drive and ability to control a project. We feel that Dr. Capa is management potential and we would like him to come over on the management side of the ladder. However he has, up to now, preferred to stay on the scientific side; his objective is to get a position similar to Le Sieur's.

Dr. Edwards is older. He works effectively in an assigned area. Because he had the technical background and because we wanted to see if he is management potential he was made project manager for the Alloy B crash program.

After talking with Dr. Thompson the casewriter wanted to get some indication of the feelings about rewards for scientific work and the dual ladder from some people lower in the organization. He talked first to Jack Redmond, a group leader in the physical testing group. Dr. Redmond was also acting as liaison man for the PTG and Dr. Berger.

REDMOND: The scientific ladder does not go that high. It's pretty obvious that the guys on the administrative side go much higher. Generally the dual ladder seems to be a window dressing. This is not only true here, but I guess in most companies. You don't see a scientist in a higher position than Berger. I don't think that there is anyone in the whole company higher than that. I don't think that he can go much higher.

CASEWRITER: You don't think Berger can go higher?

REDMOND: I don't see anybody on the scientific side higher than him. (Pause) I guess Berger's position is where the scientific ladder could lead you. I guess Berger likes what he is doing and is getting very well paid for his work.

CASEWRITER: Where do you think you would like to go in the organization?

REDMOND: I guess, like everybody else, I would like to go as high as I can. . . . I don't know yet where I'll end up.

Dr. Edwards, a senior scientist and a project manager, had this to say of the dual ladder.

EDWARDS: In theory it's supposed to go to Le Sieur's position, but in practice it stops in my position. This company has been trying to get a scientific ladder going for ten years but it has never really materialized. Those who are willing to accept managerial responsibilities receive the major rewards and promotions; the others do not. My present supervisor is a man who started working here as a chemist after I was made a senior scientist.

CASEWRITER: What are your goals now?

EDWARDS: To contribute to the company by doing research. (Pause) But if I get involved in more of the supervisory and administrative tasks I will not be doing research, so I might as well switch goals and try for a promotion on the management side.

Another scientist said: "They don't seem to be really interested in promoting for scientific accomplishment. When I see some vacant blocks on the scientific side, I will believe in the scientific ladder. But the only vacant blocks in the organization chart are on the management side and that's where the promotions are.

"Thompson, Bradford, and Kronig are good scientists, but they are all higher on the management side and not on the scientific side. I would put more rungs on the scientific side that would give positions of comparable levels to Kronig and to Bradford. They started something like this in another division, but that has been killed, so I guess it's just companywide policy not to do this.

"This problem of not having empty positions above my level on the scientific side is also felt by the younger scientists who want to be senior scientists. All that the company can do is to increase the number of senior scientists and proliferate these positions instead of promoting those who are senior scientists to a higher position so that the younger men would feel protected."

Several research managers had indicated to the casewriter that they believed that Bob Capa had management potential and that they were trying to get him to come over to the management side of the ladder. Dr. Capa told the casewriter that he was really quite undecided at this point; that he felt he could go up on either the scientific side or on the mangerial side. But he was confused because he didn't really know what that managerial end had to offer him. To Capa, a career in management seemed full of uncertainties. Furthermore it seemed to him (1) that the turnover of laboratory directors throughout the company had been very high, and (2) company policy in regard to senior men in high positions was very unstable. Dr. Capa continued:

CAPA: Supposing I was director of research and for some reason or other, they fired me. Where could I go to find another job as director of research? I have seen two candidates come in as job applicants the last year—who had been directors of research, and at this point were willing to go back to the bench—but no one would hire them. As a scientist with papers to my credit, I can get a job anywhere. I have security. Furthermore, I know what I am contributing as a scientist; I know what I am doing for science and for humanity. But as a research director it becomes much more nebulous. What am I contributing?

Also, as a scientist with publications and patents I have quite a bit of power within my own group to decide what to do. But what power do I have as an administrator in a middle management position?—I don't think

very much, at the present. I think a lot of these fellows have started to climb up on the managerial side and are not really thinking through what it would feel like when they got up there. So I don't really know which way I'm going to go on.

I think that if there was more certainty on the management side I would go up the management side, because I like to deal with people and I like to have the feeling that I'm creating something; that I have control of things. But for the time being I think I'll just stick and see what happens in the near future. (Pause) What is more useful, the dogs pulling the sled or the driver directing their efforts to a common direction? I don't know. I guess the driver. (Pause) But a research director here is more like a guy who doesn't know where he's going and hopes the dogs will get him home. All he does is prevent the dogs from fighting. Are you creative in this role? I don't know.

Dr. Berger was the senior scientist who was considered by many the most creative scientist in the CRL. The casewriter talked with Dr. Berger on several occasions over a three-week period. It seemed to the casewriter that Dr. Berger was quite concerned about the question of the dual ladder and rewards for scientific work. The following material represents several interviews with Dr. Berger over a three-week period.

BERGER: When I was hired, I was told there was a scientific ladder and a management ladder, but if I am hired at the top of the scientific ladder, then for me there is no scientific ladder. Now, I always assumed that the scientific ladder was there; that there was just no one up there because there was no one good enough to fill the slot and I always thought that if I were good enough, I'd be promoted into a slot. However, as I see it now, all the people in the higher positions in the scientific side are put there as a way to get them out of the line positions, a way to get rid of the inefficient ones. This is not only true at CRL but it's true in the divisions. I do not see anybody in the organization at a higher position on the scientific side except a failure on the management side. This means that one does not rise on the scientific side by being a scientist but by being something else.

CASEWRITER: How about a position at Le Sieur's level?

BERGER: Le Sieur's position is largely due to his special history. To some degree this is a position I could shoot for, but it would have to be different because I would not want to be so far away from research as Le Sieur is. I think I am as far away from the bench as I can be and still call myself a scientist. I wouldn't want to become an administrator. I get my joy in life by solving problems.

The question is, how can you do what you like to do and get a reward and the recognition from the company for doing this? There's no natural progression on a merit basis from where I am to where Le Sieur is. People who are in Le Sieur's position have no control over policy.

There should be several unfilled slots between the two positions, since there are several positions on the administrative side. The top men on the scientific side are all men who have been sidetracked out of management positions, so that in reality my position is the highest position a scientist was promoted into. As a matter of fact, most people in my position at CRL were hired into that position rather than promoted into it, so the scientific ladder is really a sort of fiction and a pigeonhole.

CASEWRITER: So where do you go from here?

BERGER: It's very simple. I get promoted or I leave.

CASEWRITER: What position would you get promoted into?

BERGER: I don't know the title, but I know what the job would be like. (Pause) I'd have a slightly larger group and a lot more freedom to decide what to do, freedom to go to any meetings I felt were important—within reason—and not as presently, having to restrict myself to one meeting because all the other people at my level could only go to one meeting. I also would want a voice in formulating CRL policy and the opportunity to act as an internal consultant.

CASEWRITER: Would you accept a line job if you were asked?

BERGER: Yes and no. If there was no chance for advance on the scientific end, I probably would accept, and I'd probably do a lousy job because I can only do a good job on something I am really interested in. And I'm not really that interested in the administrative side. On the other hand, the scientific ladder has fossilized very rapidly, to use a bad metaphor, and anyone who is on that side is stuck.

What I basically want is for the company to commend me for any contribution that I have made. I like prestige and recognition as well as anybody else. If I make a contribution as a scientist, I should get recognition, and I should get higher pay. I know how they feel about me as a scientist but not really as to my efficiency as a scientist within the organization, if you get the distinction, and I don't know how they feel about me in any other way.

If I wanted to become an administrator, say I changed my mind, I don't know whether there would be a slot for me or not. I would like to find out how they feel about my work and what my future in the company could be. When I had a performance review what I wanted was more of a promise than anything else, and at that time I sort of got it. I was satisfied for awhile. Then the company made a whole round of promotions and here I am, unhappy again. Now I want to know why I haven't gotten a promotion. Is it because they don't feel I am worth a promotion? Or because there is no possible slot for them to promote me into scientifically? Or because I incurred the enmity of management because I was too outspoken about the semi-metals?

In any case, whatever any of the reasons are, in order to maintain my integrity as a scientist, and my faith in myself, I have to quit if they don't promote me. When I had my performance review I got the idea that the

top position that I could go to would be Le Sieur's position, and that in order for me to get there I would have to be raised to that position in small increments over a long period of time. But I disagree with this philosophy entirely. I feel that if a man is making certain contributions to the company and he is worth something to the company, then he should get that recognition right away. So if I fill a certain slot by my contributions, I want to have the title and the money. (Pause)

I am giving up something by working here; I had a possible career in the academic world, so I want either money or a certain degree of fame. If I contribute in industry, it means that my scientific fame will be lower and that I will be less able to make scientific contributions. If I make this compromise with my scientific goals, I want to be paid for it. The company prestige and the money actually go hand in hand, and I want the prestige to get the freedom and the power to protect myself, to pick projects that I want to do.

CASEWRITER: Is the money important in itself, or is the money important as a symbol?

BERGER: The money is important as a symbol. I don't need the money. We are living way below our means right now. They may not know that I feel this strongly about this, but they know that I have these feelings because I talked to several people in the organization about this, except to Dr. Thompson. I can't approach him because I have no emotional rapport with him. If I did talk with him, I feel I might get very emotional. But I did talk to Peter Kronig when he got promoted into his job and I think he knows how I feel. It has reached the point where these considerations are restricting me and preventing me from doing my work.

Take this thing of meetings. When I first came here, I went to two meetings a year. Now I am down to one meeting a year because there is a company policy that people at my level can't go to more than one meeting a year. It's silly. I should be going to these meetings and getting contacts with people at my level and getting ideas. But if I didn't have this compulsion to cooperate with the company, I would not be doing all these projects that are in the company's interest. Part of my fight for promotion has been to get the freedom to do what I want to do on a longer-range basis.

About a week later Dr. Berger told the casewriter:

BERGER: By the way, I have had another talk with management about my position since we talked last, and a lot of that has come clearer now. I talked with a lot of people, including Thompson, and now I see what their problem is and I think they know what my problem is. Their problem is that they have nothing on the scientific side between mine and Le Sieur's position. (Pause) But what I do want is official recognition from the company. (Pause) It is not a question of money but of official recognition for the contribution I made.

The trouble is in getting official recognition. I know that the money comes with the title but I don't care about the money: I just want the title. But I know that the way they are set up, if I get the title I am going to get the money. When I talked to Thompson I resolved some of this in the sense that I can get more fringe rights: I can ask for more in other areas than the title allows.

For instance, I can go to all the meetings that I want to; I have more freedom of action in the research that I do. But my main dissatisfaction is still in the line of official recognition for the contribution that I made, or that I think I made, to the company—so I need the title.

I know what happens if you don't get promoted in this place. People all around you get promoted, and they start saying "Something must be wrong with that guy because he hasn't been moved in quite a while." And once that happens it influences everything all around you.

I would like to find out what the problem is in giving me this official recognition that I want. They feel that I can do the work and apparently they feel that I am *the* research man in the company. I would like to feel this in other ways than through informal pats on the back. I feel that I am still in a dilemma, presumably one gets promoted in this company on the basis of performance and potential. I want to find out what the reason is that I have not been promoted. I don't think it is lack of performance, so it must be lack of potential. If they feel I have no potential I want to know it.

I even had a talk with a corporate R & D administrator today. He said that if someone does not want to be an administrator, the potential for promotion in this company is limited, so the question I have to ask myself is: Shall I look for another job, that will promote for the qualities that I have and for the kind of job that I do best?

CASEWRITER: How did the talk with Thompson help?

BERGER: He may not have meant it but I have the feeling that he put the carrot on a stick in front of my nose. Essentially, I got some promises. If I work harder at more things that prove productive, maybe I will get what I want. (Pause) And this is so different from the way they treat people on the management side. Over there people are given a slot with higher responsibilities before they are ready, then they learn their jobs as they go along. On the other hand, I was told that I would have to create my job and work in a capacity with more responsibility and more contribution to the company before I would get the recognition.

As I see it, my creativity is the value that I am to the company. It requires me to stay near the bench and not to manage people in research, something that they pay off for here. (Pause) The things that should be happening are that I should be going to meetings; I should be involved in discussions of the company and company policy; I should be learning about what the company is and how it operates so that I can know the company and do the job of proposing areas for future research the

company should be in. It is meaningless to call me in the office and say to me: "Think about areas of research that the company should be in," without giving me sufficient background.

The casewriter spent several days with Peter Kronig, the director of laboratory research at the CRL. Dr. Kronig had been promoted from assistant director of laboratory research to his present position only two weeks before the casewriter began his field work. Dr. Kronig, a large, athletic man in his middle thirties, wore a crewcut and often smoked a pipe. He had a quiet, informal, and direct way of speaking.

KRONIG: You know, it really is amazing how much besides just science and engineering we are involved with in the administration of research. To do a really good job in administration doesn't give up enough time to do all of the technical aspects. This is a problem. There really never is enough time to do all of the things that should be done. I have several jobs. First, as the administrative head of lab research I am concerned about vacations, transfers, recruiting, raises, and so forth. Also, I am the project manager for several projects—four now—and probably five in the not too distant future. Also, I am doing something which I think is very important in exploratory research; I am trying to get the fundamental research being done by the CRL increased.

Pulling out a detailed organizational chart, Dr. Kronig continued: I'll show you this chart but, as you see, it's confidential, and this is one of the things I don't understand in this company: I don't understand why this chart is restricted, but it's the policy of the company not to let a man know everything about his title below that of group leader or about the limits of his salary range within that title. So the men are theoretically not aware of the upper and lower limits of their salary ranges within their titles and not aware of what titles exist in the company. A man doesn't really know where he can go and what rungs he has to climb in order to get where he wants to go. In fact, a man is often not aware of a promotion except in terms of the salary. We also have salary raises on a merit basis. (Pause)

I can't see why we use this secrecy system. It makes people unaware of their position. I guess it could be said that this system gives us more control and more flexibility in dealing with different salaries and allows us to have a certain built-in lack of criticism. If two men in the same position are getting the same salary but one is doing a bang-up job and the other guy is doing a poor job, if one doesn't know what the other guy is making and what his title is, we won't get any complaints from the guy who is doing a better job. But I think this has particularly bad connotations because we're not particularly interested in how the man who is mediocre reacts as we are interested in rewarding and satisfying the man who is exceptional. We don't really mind letting the people who are stationary and mediocre get a little dissatisfied.

We had a meeting on a case of this sort last week. We had a guy who had gotten consistent raises every year in the past, and had titles to go with his raises, but wasn't really performing the capacity of his recent title. In other words, he had been given a new title in order to justify the raise since there was an upper limit on each job category. This man, who is a sort of "supertechnician," and is able as a technician, is not really performing in the job of a senior research engineer, who is supposed to work relatively independently, although he now has the title of an SRE and he has the salary of an SRE. This man has reached this level and suddenly his raise history in the company is completely reversed: instead of getting a raise, as he has every year, his progress in raises has stopped and he has not made a single raise in the last three years. Prior to this he had a stright-line curve with increase in salary every year, and now he's got a straight-line curve—completely flat. Now it seems to me it would have been much better to stretch out his increases instead of giving them to him at this rapid rate and then leveling off.

CASEWRITER: Peter, why do you have an upper limit in the salary in each category at all?

KRONIG: I don't know why we have this and it sometimes creates problems. For instance, I have two technicians here on the chart who we have to keep in the present job category because there is nowhere else for them to go and they are now at the upper limit of their salaries but they are really worth more to us because they are doing a hell of a good job. Now what do we do with these guys? I don't know. We possibly ought to have another category, an intermediate category, between technician and chemist.

The casewriter recalled that Charles Thompson, the director of the CRL had said of the utility of salary secrecy:

THOMPSON: Let's look at the corollary. What would happen if the salary ranges associated with titles were publicized? It is not clear that an individual having this information would be satisfied with his salary and would function satisfactorily. In the extreme, everyone would know everyone else's salary. You can recognize the difficulty that would result with such a system. Thus, without demonstration of a clear advantage that would result from such a system, we would prefer to retain the present policy. This policy provides the individual with incentives since job descriptions and requirements associated with titles are available to him in the performance review at the discretion of the supervisor. In this way the individual can see his route for advancement without having a detailed knowledge of salary ranges associated with titles.

Dr. Kronig explained to the casewriter that he had to submit the expected salary increases of his subordinates for the budget proposal and that he would "think out loud" for the benefit of the casewriter.

KRONIG: Predicting supervisors' salaries is going to be a little tough

because I don't know these people too well, but I have their histories here in their folders. Let's see. (He pulled out a folder from his desk.)

Here we have a problem with one of them. He is now on top of his category of senior scientist. He can't be given an increase because he is at the top of his range. We can't promote him for two reasons: one is that we don't have a category for that, and second, I don't think we should promote him now. He has been getting increases every year right on schedule. What happens if you stop giving him an increase? The trouble is in theory that we should be able to go in a smooth curve of increasing salary to where a man will be at his maximum level in, say, a twenty-year period.

CASEWRITER: How important do you think money is to your people?

KRONIG: That depends on the guy. I think to a technician money is very important. To a bench man, I don't think it's too important at all, except as a mark of recognition and of status. I know that for Berger and Capa, even for Guthrie, it is not important except for a feeling of recognition.

CASEWRITER: What happens if your initial estimate of a man is not correct?

KRONIG: If you have to raise your estimate up or down you do so. In some cases you suddenly get a flat curve after the man has been in the company only a few years. Also, there has been a traditional increment in salary, so that if you want to stretch salary over twenty years you have to reach a step curve. Otherwise you'd be giving the man too small increments all the time. There is a real inequity, it seems to me, in this rigid system—especially when we are faced with a dynamic raise in the initial salary as bid up by the market place.

For instance, a Ph.D. chemist starting three years ago went for $800 a month, and now he's up to $950, which is more than then increment given to a person hired three years ago for that same position. The man hired into a position now is making as much as a fellow who has been working here for years and doing his job well.

For instance, we have a young supervisor who is not getting very much more than a Ph.D. we would be hiring in at a much more junior level, and this man would have much less responsibility, so where is the money incentive there?

What we try to do now is to spread the range between the good people and the mediocre people so that a good man will make considerably more than a mediocre man.

CASEWRITER: Do the mediocre men know that they are getting less?

KRONIG: No, but he will know from his performance review that the company thinks that he can do a better job, and he will be getting his raises at a much slower rate. The trouble you get into in this business is that you know damn well that the supervisors have people that are not too

good but that are rated fairly well because the supervisors are reluctant to lower the raises or to give a bad report. They don't seem to want to lose any men at all because they seem to feel that a bad hand is better than no hand at all. They feel that if they lose a man the work won't get done. The supervisors have a real resistance to turnover.

And I think there is more to it than that. You have to consider the man who has been going along without a performance review, getting yearly raises for a number of years, and suddenly they stop his raises and you don't give him any more. The question is, how much are you to blame for this, and have you been doing your job of counseling this man and being this man's supervisor? (He looks at the list.)

It seems to me that we have to get a new job category to fit in at a level above the senior scientists and research supervisor so that we can give recognition to a senior scientist who has been doing a good job. If we don't give him as much of a raise as he had last year, it will seem to him that we think we're not satisfied with his performance. He doesn't know that his category has an upper limit or that he's almost reached it. We can't explain this to him so it seems to me that we have to argue that he has to get at least as much of a raise as he got last year, especially since he has been doing such a good job. And the only way we can do this is to create a new category. (Puts folder back in desk.) Let's break for lunch.

The casewriter's notes of talk during lunch with Emil Bradford and Frank Perren, supervisor of personnel, follow below.

We discussed the personnel counselling system at CRL. Frank Perren mentioned that he had sent some personnel counseling forms to some supervisors without having received the previous ones. Some supervisors had been delinquent for more than a year in filling out their forms on the personnel counseling.

CASEWRITER: Frank, does this mean that counseling has not been done?

PERREN: It could mean that. It could also mean that there is an aversion to filling out this kind of form.

BRADFORD: (Interrupting) I can't understand why there is no follow-up when the counseling forms are overdue several months.

PERREN: I don't have the line authority to follow it up.

EMIL: In that case you should go to the man's supervisor and say, "Why don't you see what you can do about getting these forms back to me—because these men have not been counselled?" If you don't get any satisfaction from that supervisor, then go up the line. You could come to me about this kind of problem and I could tak care of it. This is a serious problem and it shouldn't happen.

After lunch the casewriter asked Peter Kronig to elaborate on the performance reviews.

KRONIG: The way I do this is to do the paper work on it by myself first and then review it with my supervisor, in this case Emil Bradford. Then I have a meeting with the man and discuss with him different aspects of his performance and also work out with him a plan to better his performance in the future. We call this the interview. It is supposed to occur once a year on a man's birth-month. I also get involved in these reviews as a review agent for many interviews. Theoretically, I would have to review seven men directly under me, plus the nine men directly under them, and all of the men who have no intermediary between themselves and the supervisors. And this is quite a few men. Theoretically, I would review all of Capa's subordinates as a review agent.

And this is one of the problems. I've been trying to get Capa to train an assistant and I've been having quite a lot of problems to get him to accept someone. He is a man who works in the style of a man who wants complete control of the projects. He hates to delegate authority. When I asked him to get someone as his assistant, he turned to me and said "Who is there in my section that I can name?"

Sure enough, looking in his section, there isn't anybody that I can name. Well, he's coming up for review and I have this in his report and we have talked about this before, and I think at this point he recognizes the problem. But he just won't let go of the reins. We have told him, in fact, that he should find himself an assistant.

There was a man who we felt was competent who was hired as a senior chemist to work in Capa's group, but within three months Capa had managed to get rid of him, saying that he was too lax. And this might have been, but Capa expected too much too soon. So I told Capa that he would have to go to Frank Perren [personnel supervisor] and ask him to find him an assistant and simply tell him what the qualifications for this man would have to be. This is to get Capa to say exactly what he wants. Once it's on paper he'll realize what he's asking for.

Now Klause is another person I have to review. Klause is a very precise man, has a rather narrow viewpoint, likes to have a job in which there are specific outlines, specific goals, and will have a tendency not to communicate. As I said, he likes to go off for a time and just do the job. I've tried to tell him that this is not possible when there are many people dependent on the results he can produce. I think he has come around a little bit in this direction.

CASEWRITER: Peter, how do you evaluate the evaluation that, say, Klause has done on someone under him?

KRONIG: Well, we had an instance like this just the other day. Klause came in with an evaluation and apparently he had not read the instructions or the covering letter that went out with the evaluation sheet and had filled out this form only partially—just the mechanical side of it—without

the qualifying aspects of the interview, and held the interview without reviewing any of this with me. Well, by that time it was all over, so I said "This is not really right; you should take this back and see if you can't specify ways in which your man could improve." (Knock on door and VAN DER BUNT comes in.)

KRONIG: What is the story on X division? Why are we doing this process? What good is it?

VAN DER BUNT: I don't know. I don't know their objectives, don't know what they want. But they get a charge for the service we do for them so we don't lose by it. After all, we have to balance the number of times we say "no" with the number of times we say "yes." When they request us to do a project, we have to do some work for them.

KRONIG: But still for us it means that we are spending a lot of time on something that is getting us nowhere. The only property that we ever found that was interesting in the sample is that it looks like it would bond to glass. Let's investigate this property and stop working on tensile strength and all that stuff.

VAN DER BUNT: Well, I went ahead on the assumption that we do quite a bit of work for these people.

KRONIG: You will do quite a bit of work. You submitted a research proposal that looks very good, so let's do some of this new work of yours.

VAN DER BUNT: But who is going to make the alloys that I need for my research?

KRONIG: You don't need anyone to make the alloys. You can buy them. Who would you put on this project of yours?

VAN DER BUNT: I don't have anyone. I need someone like George.

KRONIG: Why can't we get George? Isn't there anyone who can replace him on what he's doing now?

VAN DER BUNT: No, he's needed for another project. (Pause) I'll have to go to Chicago to see our man there. (He talks about a consultant at Chicago.)

KRONIG: While you're at Chicago why don't you go see this student that we want to get.

VAN DER BUNT: I could.

KRONIG: You really should. We don't want this guy going some place else. What about the other people we have on tap?

VAN DER BUNT: I can't give more time than a couple of days a week to this project of going to the schools. I've been doing this for six months now.

KRONIG: Yes, a couple of days a week is just about right.

VAN DER BUNT: I will be away tomorrow and so will Bob Steinpilz and some other people. (He goes out, and Kronig yells after him.) Who's going to be in charge?

VAN DER BUNT: No one. Your secretary.

KRONIG: No kidding; who's in charge?

VAN DER BUNT: Your secretary.

KRONIG: I'm not kidding; who *is* in charge?

VAN DER BUNT: Well, we'll put Lee in charge. (Walks out.)

KRONIG: (To casewriter.) Just trying to get him to think ahead about bringing people along.

Chapter 23 (Continued)

*AMERICAN METALS COMPANY (E)**

One morning during his stay at the Central Research Laboratory the casewriter was sitting in the office of Peter Kronig, the director of laboratory research at the CRL. While Peter's secretary was making coffee for the two men, Peter was making arrangements to receive a new man.

Kronig: This is a new man for AMC. We had hired him with the idea of having him start in Berger's group. Yesterday when he was in he thought he wouldn't work with Berger but might prefer to work with Capa. I talked with Capa yesterday and I have to talk with him again today to find out what he has decided. (At this point Frank Perren, the personnel supervisor, came in the office and handed a paper to Peter.)

Perren: This is a theoretical physicist who was at A division for an interview. They want to know if we are interested enough to call him back from Boston for an interview.

Kronig: Why didn't we see him when he was here?

Perren: He was here and gone before we knew.

Kronig: Doesn't A division want him?

Perren: No, but they think he's good.

Kronig: Too bad. Let's talk later about this. (Frank Perren leaves. Peter Kronig turned to the casewriter.)

Kronig: This is a bad situation where they fly a guy all the way from Boston and then send him back and don't give us a chance to talk to him. (Looking at the application form and smoking his pipe, Dr. Kronig continued.)

Hm, exploratory research. That's interesting. This looks like a fellow doing very, very fundamental work. I wonder if he would come to work for us? He seems to have been high-rated on the technical aspect and on creativity, and rated medium on personality. (Puts the forms away and picks up the phone.)

Hello, John? Are you busy now? Can you come down? (He hangs up the phone.)

That's John Stein. He's in charge of one of McDougal's groups while Mac is away. What I want him to do is to review a series of publications

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by a guy who is looking for a job. I want him to evaluate the quality of the papers. This is something that Emil Bradford asked me to do. (John Stein comes in; Peter asks him to evaluate the papers.)

STEIN: You want me to evaluate the papers or to evaluate the guy?

KRONIG: No, I think they want to evaluate this man. Just give me an idea of what you think of the quality of the work. Let me know today or tomorrow what you think.

STEIN: Is this the guy who was around here last week?

KRONIG: No, I don't think he was.

STEIN: I think there was a man around here last week. I think this was the fellow because he was in the same field. It seems . . .

KRONIG: I don't think so but let me check. (He picks up the phone and calls Frank Perren. After the conversation he turns to Stein.) No, this is not the fellow who was around last week.

STEIN: Was that Frank Perren?

KRONIG: Yes.

STEIN: Well, you should have let me finish before you called Frank. This guy didn't come through Frank. He came through A division. I think there was some kind of a mixup on this thing. A division sent this guy over to us to evaluate him because they didn't have the technical background to do this job.

KRONIG: Well then, so much the better when you have an interview with this guy. But look at these papers anyway and tell me what you think of them. And before you go let me ask you about this. Here's a guy from Boston who was in to see A division. Does his background have any interest for you? (He reads the list of publications that the job applicant had published while a student.)

STEIN: I don't think so, not really. This guy is too theoretical.

KRONIG: Well, we'll ask Berger if he is interested.

STEIN: He will be. (Pause) What's his experience?

KRONIG: None. He's just out of school. He was rated very high on technical ability and creativity by A division.

STEIN: (Looks at form.) Wait a minute. Maybe he is just what we are looking for. If he's willing to change just a little bit, his background is just what we need . . . if he would go just a little bit more on the practical side.

KRONIG: So this guy could, with his background, take over your job in the future?

STEIN: Yes, his background is sure good enough.

KRONIG: Probably nowhere in the company do we have this kind of background, do we?

STEIN: No. Say, can you expedite this thing? He seems to be just what we are looking for. I sure wouldn't want to miss out on him. (Stein leaves.)

KRONIG to the CASEWRITER: I have a chore to do now, my interim report for the last six months. (Pulling out some papers, he started to look at them. At this point there was a knock on the door and a man came in. He was introduced to the casewriter as a new employee of the CRL, Dr. Newman.)

KRONIG: Did you talk to Capa?

NEWMAN: Yes; I got a pretty good idea. He's got a very large group there, hasn't he? I have a feeling that Berger's group is really where I belong, especially in view of the personnel situation.

KRONIG: Well, in that case we'll assign you to Berger's group. If you have any ideas you are working on, just put them down on paper for Berger, with a copy for me and Dr. Bradford. Don't feel you have to do this right away. It'll take you a while to acclimatize, but if you get any ideas just put them down on paper.

NEWMAN: Oh, yes, I'll do that. Berger's group is really the place for me; the fundamental group, that's really where I belong. There's a variety of projects to do there and I think the work is much more interesting.

KRONIG: How are you doing for living quarters? Do you want to take a day off to look for an apartment? What we have to do now is get you situated up in the lab, get you a desk, and so forth.

NEWMAN: So you'll assign me to Berger's group?

KRONIG: Yes. I expect you to work on ideas that you get in that group and I think you'll work out very well there. (Newman leaves. Peter Kronig turned to the casewriter with a folder.) Here is an evaluation of three of our alloys done by an outside laboratory. I have to look at this and then send it on. As a matter of fact, I'll send it on first because the guy who's got to see this is going on vacation next week, and I want to see him before then. (At this point he gets a phone call.)

KRONIG: Yes, John.

STEIN (on the phone): This guy we talked about is a very good speaker. He has a group down there. You might get a line on him through Kartofel at CIT.

KRONIG: Yes, we can ask Harry to ask Kartofel what he thinks of him. Thank you. (Hangs up and turns to casewriter.) Here is a copy of the last interim report. This lists our program of projects we are working on and what we expect to work on in the coming year. This comes out twice a year, as a budget report and as an interim report. Actually, it gives in capsule form what we are doing for my use in preparing the section's budget report that comes up in six months. Now, at 10:45 I'm supposed to talk to Dr. Lion, an applicant. He's going to be a tough cookie. He got his Ph.D. way back in 1941. (Receives a phone call, then says to the casewriter:)

This is a bad situation. I'm supposd to prepare a monthly report but I can't do it until the supervisors hand me their reports, and the supervisors

are supposed to have their reports in by the 15th. I'm supposed to have mine in as the project manager by the 27th. I can't do anything until this one supervisor gets in his report because this can't be done until I have all the supervisors' reports, so I'm waiting on Carl for it. He's late again. Ed [the former associate director of laboratory research] talked to Carl then but I don't think it did much good. I guess I'll have to do it again. (Picks up phone and calls Carl.)

KRONIG: What? He's gone on vacation already? What about that monthly report? Okay. Call me back. (Picks up Lion's folder again and studies it while smoking his pipe.)

Let's see now. He starts with [high reputation electronics company] in the fundamental research lab and after four years he's with [high reputation diversified manufacturing company], then more and more applied stuff with (two lesser metal companies) and now, good Lord, Atlas Corporation. It seems to me that this guy is progressing to more and more applied stuff. I see he came here in 1960 looking for a job; I wonder what the comments were at the time. (Picks up phone and calls Perren.)

KRONIG: Frank? Peter. I have an application from Lion in front of me. What were your reactions in 1960. (Pause) He never came? They *think* he was here? Nobody seems to know, eh? (Hangs up phone.)

Apparently he contacted Stone directly. There's no record of his being here whatsoever or any record of what happened, or whether we made an offer to him or not. He's with Bradford now. This is an example of what state our interview system was in before. This man contacted Stone and came in for an interview, but whatever happened no one knows.

When I first got here, the system used to be really haphazard; there was no formal schedule or circulation of applications before an interview. Sometimes key people were tied up and interviewees just sat around waiting. I think we are getting a little bit more organized now so that we are able to handle an applicant more systematically, and he gets to see people that he should see. We do not go overboard, as some companies do—who actually have someone take the applicants out and wine and dine them exhaustively—but we are a lot better than we used to be. (Puts down file and picks up another one.)

This guy will come in this afternoon. Just graduated. He apparently doesn't know what the going rate is. He's applied much too low. Well, that doesn't matter, we'll boost him up if we hire. Gee, he's been unemployed for six months and now he's working in an IBM conference. I don't understand this, there's something strange here. I'm going to have a hard time finding a place for a B.S. (Picks up organizational chart and looks at it.)

CASEWRITER: How does an application like this get to you?

KRONIG: It depends. We have a regular recruiting program at the colleges and universities. At the college interview we make a preliminary screening. The people at the corporate personnel office, along with one of

our technical people, hold the interview. They know the personnel requirements of the divisions and of CRL.

The applicant fills out a form at that time. Then the personnel office may send the application to me and we arrange for the interview. Actually, Perren is the man who arranges most of these things here at CRL. He looks up the qualifications, checks on the references, and works up the interview on a scheduled basis.

In the case of this young B.S. we got the resumé from an agency, directly. Right off the bat we suspect something because in this business there is no reason for a man to use an agency to get a job. Then we see that this guy has been unemployed for six months. We would have to find a good reason why this guy was unemployed, like he's been to Europe or something. Otherwise, there's no reason in the world why a B.S. couldn't work if he wanted to.

(At this point there is a knock on the door and Dr. Lion comes in. He is an older man and seems rather nervous. The casewriter is introduced to Lion, who says he has no objections if the casewriter listens in.)

KRONIG: I understand you were here at CRL a few years ago.

LION: Yes, I saw Stone. Actually, I came in through the back door, through some connections.

KRONIG: Who did you talk with then?

LION: I talked with Capa, with Dr. Le Sieur, and with W—— [formerly a director of the CRL, now with a division].

KRONIG: It seems to me that most of your work was in the area of solid state physics.

LION: Yes, in all phases.

KRONIG: As I understand it, over at [another metals company] a man would be in a project all through its life, so he would do synthesis, analysis, and so forth; all parts of the process.

LION: Well, at the time I was there the lab was split into two groups; so at that time this was not entirely true.

KRONIG: At that time, were you interested in a new process for rare earths?

LION: No, I guess I'm not telling you anything out of school; that's all history now. . . .

(He describes the process that the company he worked for had developed to get around another company's patents. Kronig asks a number of other questions centering around Lion's work in the various companies he had worked for and then asks him why he had gone to the Atlas Corporation.)

LION: They have an excellent reputation for personnel and company policy and they're one of the best machinery companies in the world. Recently they started to try to diversify and one of the ventures in diversifying was making a special kind of alloy. In this venture I was doing the analysis for producing the alloy. This exploratory work was

used both for the synthesis and to develop processing machinery. But the company's objectives changed and the program was dropped, and I found that I could not stay in the company and make any contributions.

KRONIG: It says here you were a group leader at [one of the other companies Lion had worked for].

LION: I had two or three people, on an informal basis. To tell you frankly, I didn't like it there at all. That's why I went to Atlas. It looked at the time like a fine company.

KRONIG: Did you supervise any people there?

LION: No, just one guy. There was a guy there with a B.S. degree and I sort of led him along. But you know, I didn't do too much physics at Atlas, and that's what I'd like to do. I'd like to get back into physics.

KRONIG: Did you have any freedom to do what you wanted?

LION: No. The direction was much too mechanical.

KRONIG: What are your objectives now? If you came here to AMC?

LION: I guess you call it a senior scientist. I've always been a bench man and what I would like to do is go back to the bench. The last two jobs I just haven't been doing any physics.

KRONIG: Does this mean that you don't want to be on the managerial side?

LION: No. I always wanted to be on the managerial side but I just never had the chance.

KRONIG: Here, a group leader is a working physicist who has a managerial responsibility part of the time.

LION: Yes, that's exactly what I want. That's the kind of job I want.

KRONIG: Well, as you know, we are somewhat committed to the rare earth side of metallurgy and this fairly closely approximates the work which you have been doing. Now let me see who you'll be talking to. (Looks at list.)

Your list shows only people on the managerial level, but I would like to have some people talk with you who are on the supervisory level and who are doing work on alloys. So I think while you are here I'll try to arrange for you to see some of our supervisors later this afternoon.

LION: That's all right. I have lots of time. I'd be delighted to talk to supervisors.

KRONIG: On this question of free time. We expect that people will want to do some work on their own, but we have no formal free time as such. We are flexible, so that people who work best on their own are given more encouragement to do so, and people who are mostly involved with routine characterization or applications work do less exploratory work. Tell me, would you prefer to do long-range projects or short-range projects?

LION: Oh, short-range. I've gotten too industrialized to work on a blue

sky project. I don't like to work alone on a project. I sort of like to feel the results of what I'm doing and like to feel that the company is getting something out of me.

KRONIG: Over at [another metals company] one man does all the aspects of the work on his project including applications and development work. Would you consider doing only applications work here?

LION: Oh yes.

KRONIG: I want to see if a couple of the supervisors I would like you to talk to are available. Berger and Capa, I think. Let me investigate with Perren if I can set this up. I wonder if you would consider doing applications work exclusively; that is, not synthesis but just the applications, giving the projects a physical point of view, so to speak. Our applications work has been going at a very low level and we recognize this and feel the need to increase our efforts in this direction. Let me ask you, do you have any questions about us?

LION: No, I know pretty much what the work is about. I guess you know that I have been to quite a few places in the last few months. I think this is one of the half a dozen places I would like to work.

KRONIG: Let me ask about timing; when could you decide?

LION: Pretty quick—once I make a choice—I think.

KRONIG: Any reason why we should act quickly on our side?

LION: Yes, I have been traveling around for a month or so and I'd like to get settled. You know, I really would like to get settled in this area. This is a fine area, and it has always struck me as one of the areas best suited for a man in my field.

KRONIG: If you have any questions later, even after you leave, please pick up the phone and ask me or any person with whom you talked for an answer. Don't have any nagging doubts about anything, about company policy or anything like that.

LION: I'm pretty well industrialized. I don't care much about company policy or rules. I just accept it and do my work. But about this timing, I think I could move pretty fast if it came to deciding.

KRONIG: We can, too. I think in this case we will. (Lion exits. Peter Kronig turns to the casewriter.)

This guy is something of a special case. He is too long out of school to be able to be quizzed on technical aspects without going into proprietary secrets. Most of the stuff he has been working on has been proprietary. His thesis is too long ago. I feel that if we make him an offer, he will accept. I think he might fit into the applications work and could, as a physicist, contribute something to that group. The applications group could use a physicist's point of view. One of the problems is that we really don't know where the applications group is going. We have to decide first and pretty soon. If we make this guy an offer we have to be prepared to deal with an acceptance. I think what we would get out of this guy is experience more than anything else.

After breaking for lunch, the casewriter was again with Dr. Kronig in his office. Dr. Kronig began by talking about the problem of recruiting Ph.D.'s as bench scientists.

KRONIG: It is really a tough problem because the demand is much greater than the supply. This means, of course, that we have to increase starting salaries in order to meet the shortage of supply, and this in itself creates a problem with our older men. We also have a technical problem in getting the kind of expertise that we want in metallurgy. The work in industry in this field far exceeds the training that the universities are providing. I expect this situation to ease off as we build a better reputation as a lab and can point out our climate to the new people coming in. As AMC becomes recognized as a good place to work in fundamental research, I think we will have a much easier time in recruiting top-level guys.

We have several things going for us that we try to sell. One is the plant. The other is the opportunity for advancement here at the company. Then we have a very young group of managers and supervisors who are trained in a newer physics. I don't know if this has any meaning or not, but I think that young people coming in like to be able to discuss their new ideas with people they feel understand what they are talking about, and I think they would like to work for people who are also trained in the newer approaches to metallurgy. (Pause)

I think I'd better work on this rating form on Lion. [See Exhibit 1.] I like to do these forms before I talk to anyone else about the man that I interviewed. (Studies form.) This damn form is so rigid.

CASEWRITER: How do you say "yes" or "no"?—this man should or should not be hired?

KRONIG: I guess in a sense what they are trying to do is to make us commit ourselves to say yes or no, because this essentially is what the laboratory has to decide—yes or no. But it's damn hard to do. The way this works is that all the sheets are collected from the men who interviewed the candidate and they are then reviewed by Frank Perren, Emil Bradford, and myself, separately, and then they go to Charles Thompson. The supervisor of the group where the man will work also rates this man, and his form also goes to Thompson, who then makes a decision. There's no formal meeting in which the man is discussed by several people. This sheet in a sense does this.

Peter Kronig fills out the sheet except for "Hire: Yes or No." He looks at it for a long time, puffing his pipe, looks at the organization chart, looks back at the rating sheet and, after about five minutes, turns to the casewriter.

KRONIG: I don't know as how I can fill this thing out. (He looks back on it and after a while puts a check on the "Yes" and an asterisk for a comment. The comment reads: *Could do competent job as senior research*

physicist with upper limit as a group leader. Should work in applications group. Has a wide background, experience; creativity questionable. He turns to the casewriter and says:) I might be all wet on him. I just don't know.

CASEWRITER: Does this form help you make an evaluation?

KRONIG: In one sense, yes. It makes me do the thing right away. I don't think it helps to push this kind of thing off. I think also, without the form, some people would be influenced by what other people said. In this way, if anyone gives the man a bad rating, then the man is not hired. It seems to me this fellow should go into an applications group because of his experience. That's essentially what we're buying, his experience.

You know, this applications group is not really formed yet. I don't know where it will go; it may go under me here in the lab research, or it may not. Some people feel that applications should logically be after development, but I think it should be with the economic analysis group, or in our group, because we should have the applications at the lab end so that we can get an earlier feedback and either drop projects or alter them early in life.

(Dr. Kronig is interrupted by the telephone.) Yes, Emil. (Pause) I'll come over now. (To casewriter) That was Bradford. He wants to talk about the man John Stein was talking to. (Kronig and the casewriter go to Emil Bradford's office.)

BRADFORD (to casewriter): The background on this is that an applicant walked into A division cold. He'd answered an ad. He was a specialist in some field of physical chemistry. Most of the people with a background in this area at A division were on vacation or were out, so they called me since I used to work there and asked me if I could set up interviews for him here at central research, so we could evaluate this man for them. I looked at this man and listened to him and I couldn't make up my mind whether he was a crackpot or a genius, so I sent the man to Thompson and he had exactly the same reaction. So we had this man submit some papers, publications, and we had Peter's group look at these papers. This man worked in the same company for 20 years and now wants to leave this company because the company changed goals and he feels quite unhappy. (Turns to Peter.) Well, Peter what have you found out?

PETER: Some of these papers are on a topic somewhat familiar to me. There was an error in some of the X-ray work that was done, a rather basic error, and I don't know if this man was responsible or not. Some of the other work is rather standard. This fellow talked to Carlos Landa and John Stein in Mac's group and Carlos says that he was a very good talker but he wasn't too impressed about what he said. This man seems to have gotten more information out of Carlos than Carlos got out of him.

BRADFORD: So at this time you can't write off the man as a crackpot?

KRONIG: No, I think he needs a further looking into.

There seemed to be a high degree of consensus among the supervisors and scientists with whom the casewriter talked about the difficulty of evaluating a candidate's scientific capability in an interview. Some of the relevant comments recorded by the casewriter:

EDWARDS: You can't really evaluate the scientific capabilities of an applicant in half an hour—or his character—for that matter. At best, we can only estimate and try to make intelligent guesses as to his capabilities and qualifications for a given position. After several months, or even a year or two on the job, some develop and become valuable assets; others do not.

BERGER: Well, what we can't pick up in these hour or half-hour interviews with the man is orginality. I don't think there is any form in the world that will pick that up.

CAPA: One of the major problems is how you can judge a job applicant in half an hour. You can't. It's even tough on the technical side because everybody is so specialized. You hire a guy and you find out only a year later that he is no good. I was trying to do something in this respect. When a job applicant was coming in, I got hold of all the publications he had made and I circulated them around so that at least people could read what he had written. But this wasn't done by the company as a whole, and after a while I stopped doing it. But this is still one way you can tell how good a man is technically.

There is no feedback from personnel on what happened to the man after the interviews. I seldom know what the final disposition of the case was. All that I see is a stream of applicants who I rate. In some cases a man who I rated low suddenly appears for work, and, in some cases, even in my own section. It even happened that people were hired into my section without my ever having interviewed the person.

Another SUPERVISOR said: The trouble with the forms is that it's not only the applicant who is being judged, but the evaluative ability of the evaluator. It's pretty hard to fire a guy when six people, including Perren and Thompson, have their signature on a form saying the man should be hired. So there is a tendency to let mediocre people vegetate.

The interviewer asked Dr. Capa how he had rated Dr. Lion.

CAPA: Oh, Lion. That's a sad case. There's a man who stopped creating the minute he left school. After all, this man had been working for some of the top companies in the country. By now he must have established very good contacts. The fact that he wanted to come back to being a bench man, and was not able to get back into a good company, shows that the man was not very creative. Even if he had been only medium they would have taken him back because, after all, he put down on his application form that a vice president of [highly regarded electronics company] was his former group leader. Well, a man who is vice president can put a guy to work even if he's only average. So I don't have very

much trust in him. Also, when I talked to him yesterday about the technical aspects of the work he was supposed to be doing, he came out with very bad answers. This showed him to be a very poor physicist in these fields. So I definitely rated him as not acceptable to the company.

The casewriter was interested in the recruiting procedure and interviewed Frank Perren, the personnel supervisor. Dr. Perren was a man in his forties who had been a bench physicist and had become increasingly interested in the administrative aspects of research management.

CASEWRITER: Frank, when you get the sheets back from people who have interviewed a job applicant, and there is some disagreement, how do you decide whether or not to hire a man?

PERREN: If there is a general interest shown in the new man by the sheets, I will check his references, usually by phone. Then, if his references check out, I try to find a place for him in the organization. Then I check with the supervisor who will be his boss. If a supervisor who might be his boss has rated this man low, I will not assign this man to that supervisor. Then I check with Bradford and Thompson and with the director this man will be working for, either Kronig or Danton, and if all of these things check out, I will telegraph or call the man and tell him that we are interested in him and are going to make him an offer. This technique gives us a higher percentage of acceptance than is true of the rest of the company. (At this point Perren took out a record sheet and showed it to the casewriter.) You see, out of 30 people we made an offer to, we have 16 acceptances, 10 refusals, and 4 undecided. This is very good compared to the general company performance, which averages only 1 acceptance out of 5 offers.

CASEWRITER: What, in general, decides your dollar offer made to a man that you *want* to hire?

PERREN: We have a general list of starting salaries around the country, broken down by types of degrees, schools, and specialities, and we try to stay in the middle of these ranges. We don't like to go to the upper ranges unless the supervisor wants this man badly enough and will back me up with Thompson and with Bradford.

Sometimes they call an applicant with an offer and we will get a feedback. For instance, in one case I called a young lady and made her an offer and she said "Well, you're not in the ballpark; I've gotten higher offers from two other people."

One of the things that I look for very carefully is what are our people making who have comparable training and experience. I can't have a new man in here making more than someone in CRL, who has been here for some time. This would be a very bad policy.

CASEWRITER: Do you feel that you lose the top ten percent, say, because of too low salary offers?

PERREN: Yes, I guess we do. But it's fairly obvious to me that the real

scientists that come in here are more interested in the type of work they will be doing and the freedom they will have than in the amount of money they will be earning.

CASEWRITER: Do you feel there was a high percentage of top people in the group that rejected your offers?

PERREN (looks at list): Well, I don't know. No, I guess it wouldn't be fair to say so, although there are some people who turned us down, who I would have liked to have had, who turned us down for reasons of salary. I guess some of them just got more money somewhere else.

CASEWRITER: Frank, I am interested in the way you work with the forms that you get from the people who interview new applicants. Just how do you evaluate the evaluators?

PERREN: Well, after a while I get to know the evaluators. They each evaluate in their own characteristic way. Berger, for instance, does a good job, and I have faith in the way he evaluates people. Guthrie does a good appraisal, tends to be less critical than the average. Klause gives only a "yes" or a "no" and no explanation. Capa tends to disagree with everyone else and is much more erratic than the others, rating much higher or lower. I want to evaluate myself; I tend to be very easy—I'm impressed with everybody. Edwards is very critical.

CASEWRITER: How do you form your opinion of the raters? How did this happen?

PERREN: This is a feeling over a period of time.

CASEWRITER: Do you arrive at this by checking back on the forms that were filled out years ago to see how people were rated and how they turned out, and who was the closest?

PERREN: No, *I* haven't done this, but it can be done. Let's have a look. (Goes to the file and pulls out several folders.) Let's take a good one and a bad one. First, Bill P. He hasn't worked out as well as expected. (Perren opens the folder and both men checked through the list. All of the people gave the applicant a good rating and wrote that he should be hired.)

CASEWRITER: Frank, do these forms go to corporate headquarters?

PERREN: No, they stay right here in the man's file. Now let's have a look at another applicant. Here's a guy, Waldo E. He's a Ph.D. and he's worked out very well, better than expected. (Looking over the rating forms, the casewriter was struck by the fact that both had more or less the same ratings in the same categories: both were rated around 2 or 3 average on the 5-point scale.)

PERREN: Here's another guy, with an excellent rating average: between 1 and 2.

CASEWRITER: How did he work out?

PERREN: I don't have any way of knowing how he worked out. He's working for Berger and Berger hasn't come to me to say that he's very good or very bad. So I don't know.

CASEWRITER: Frank, is it hard to fire people?

PERREN: No—but the only people I remember firing were technicians, and those were for good cause.

CASEWRITER: Does corporate headquarters get into the picture?

PERREN: No, but Dr. Stone [corporate vice president for R&D] you know is here once a week, so he never gets too far away from what we're doing. Now here's another man. He works for Klause. Edwards rated him at an average of 3, with a comment to hire.

CASEWRITER: Isn't this kind of strange: to rate average and then say to hire?

PERREN: No, our policy is to hire at 3 or better. [See Exhibit 1.] The average rating for this man was 3, with "questionable" on hire.

Exhibit 1

RATING SHEET FOR APPLICANTS

RATING SHEET FOR APPLICANTS

Name: Date:

Degree:

School:

	Excellent		Average		Poor
Mental Ability	1	2	3	4	5
Drive	1	2	3	4	5
Leadership Potential	1	2	3	4	5
Personality	1	2	3	4	5
Overall Rating	1	2	3	4	5

All things considered, would he make a good addition to our staff?

YES________ NO________

Comments:

____
(Signed)

CASEWRITER: How did this man work out?

PERREN: I haven't heard anything bad, or good, about him, so I don't know.

CASEWRITER: You don't get much feedback from the supervisors about the men, do you?

PERREN: The feedback I get is supposed to come from these development reviews, but I have had a lot of trouble getting some of the supervisors to get these reviews back to me. (Pause) Here's a rating sheet filled out at a college by Kronig and Danton before they had their present jobs. You see, it tallies very closely with the rating of the man when he came to work here.

Frank Perren's boss was Charles Wilson, the director of research administration. Wilson had been a bench chemist who had slowly transferred to the administrative side. He was a man in his fifties.

CASEWRITER: If five out of six people had rated barely "yes" on the Should-we-hire scale, and one man had rated "no," would you hire this man?

WILSON: It depends on who is giving the "no." Some of our people have a much harder criterion in rating than others. We have to be aware of this, also of the realities of the market place. Good people are very very hard to get. This makes us settle for less than perfect sometimes.

CASEWRITER: How do you keep track of the market prices for personnel at various levels?

WILSON: Well, we have surveys, and there are publications on this. The placement office has information and also the schools have information on this.

CASEWRITER: How flexible is your salary structure on hiring new personnel?

WILSON: We have guidelines for different categories. An offer is made, based on the average offers made to people who have accepted in the preceding year, and we can also go a little higher than this—about 5 percent for an exceptional man.

CASEWRITER: Let me switch subjects here, Charlie. The secrecy on salaries for job categories—does this originate here at CRL or at corporate headquarters?

WILSON: It's been that way ever since I've been here. Perhaps it originated at corporate headquarters, but when I came here there were no job descriptions and, I think, no formal ranges on titles.

CASEWRITER: When did this start?

WILSON: I think it was 1957. I think [a well-known consulting firm] were sent here and they wrote the first job descriptions. At that time the director of the lab was not too enthusiastic about this system, but it was imposed on us from above.

CASEWRITER: Could Central Research Lab change this system at its own will?

WILSON: I think we could, but I suspect that it's pretty general throughout the company to keep this kind of thing confidential.

CASEWRITER: Where do you think this secrecy is good, and where do you think it's bad?

WILSON: It's not good in knowing where you stand and what you have to do to get where you want to go. But it seems to me that a frank discussion with your supervisor about your work is just as good a method of letting you know where you stand as a dollar figure would be. And it seems to me that an openness about salaries and salary ranges could be a crutch by management in not talking to the subordinates about where they stand. This way, if a man does not get a raise he knows that he did not perform as well as he could have.

CASEWRITER: How can you give a raise to a man who is at the top of his range and where there is no superior position to which he can be promoted?

WILSON: We have the personnel development talks in which the man is told, two years ahead of his time, when he is getting to the limit of his ability. Also, the squeeze put on salaries by increasing starting salaries is going to help us because there will have to be a readjustment to take into consideration the value of experience. This is done at corporate headquarters from time to time. At the same time the cost of research is increasing, and when you have to justify the results—the value of the research in terms of the dollars spent—there is a continuous pressure to keep the budget down.

CASEWRITER: How is the budget decided on? Are you involved in this?

WILSON: I'm right in the middle of it right now. We come at it from two angles, with projects and with dollars, and then we reconcile these two approaches. Sixty percent of our budget is in salaries and this is a controlling factor. Projects are translated into money on a standard cost system by means of a man-month, so we will start to set up our budget in terms of the projects we want to do and translate this into a dollar figure. Then we have a rough estimate of the dollar figure within which we are supposed to work.

CASEWRITER: When the reviewing agency reviews the budget do they talk in terms of dollars or in terms of projects?

WILSON: Last time they asked "What would you do if you had to reduce the budget by 10 percent?" And we scurried back and worked out our new budget by chopping off a project, by letting go some people, and so on, all on paper. But as it turned out, they didn't ask us to reduce our budget.

Ordinarily it's much less formal than this. We talk to Dr. Stone and Dr. Humbert and they work on the project selection with us, and, of course, they have connections with the board. The board, essentially, thinks in terms of money, and when they talk to us they ask us in terms of reducing or increasing the money part. Stone translates this into what projects could be lost and what this means to the company.

CASEWRITER: Is the budget done once a year?

WILSON: Twice, really; we have the interim report between budget times. This is an informal report and is circulated within the lab. We also prepare an informal report one year in advance, so that projects are selected and proposed, stopped and increased, throughout the year.

During his final interview with Charles Thompson, the director of the CRL, the casewriter asked for his views on the process of personnel selection at the Central Research Laboratory. Specifically, the casewriter asked what function Thompson saw in the round of half-hour interviews that a candidate had at the Central Research Laboratory.

THOMPSON: The round of interviews that we have was set up to achieve dual objectives; to provide us with an opportunity to evaluate the candidate and to inform the candidate about the CRL. In the current situation, selling is as important as evaluating. This is especially true when the candidate is of exceptional quality. The system we have gives the candidate a chance to get a sample of the personnel and of the work done at CRL. All we can hope to do, it seems to me, is to make the CRL more interesting so that the one out of five who does accept our offer is a topnotch man. In addition, we have a program which gives the new Ph.D. an opportunity to discuss his work with our people in a seminar. This provides us with an opportunity to discuss his research with experienced research scientists.

The casewriter felt that a portion of one of his interviews with Dr. Kronig cast some additional light on the recruiting and hiring process at CRL. Dr. Kronig talked to the casewriter about how he saw his job in the future.

KRONIG: Fundamental research is the future of the company. I expect to maintain an interest in that. Also I think that my job in the future will have increased emphasis on training of our people to administer research, and this is especially true, I think, in our second-line supervisory personnel, where we have really a very weak position.

Another thing is in the hiring of new people; I think we have to do a better job there. In a sense, in the past I think we have too often been hiring just to fill the spot with a warm body. One way to get better people, I think, is to get a better reputation, and you get a better reputation in this business by publishing good papers and by word of mouth. The word of mouth comes back through the universities where we get these men, that we are doing good research at AMC, and I think in order to have the reputation we have to do good scientific work.

I think we have to take another look at the salaries, at the administration of personnel, and I think we want to increase our rate of turnover. We want to get rid of a few people that aren't performing. I think AMC has shown a genuine desire to improve its position. We have the money; all we need is the people to do the job.

SECTION IV
Conceptual Readings

Chapter 24

MAN AS A RESEARCH TOOL*

PHILIP A. SPRAGUE†

A TRIP through today's research and development laboratory usually is a most misleading experience. Whether the onlookers are visiting firemen, foreign research teams, or a pack of cub scouts looking for Science in Action, corporate guides nearly always present a totally incorrect picture of how innovation and adaptation actually comes about.

"And this," the vice president avers to the visitor, "is our new (long word) machine, which at a cost of $\$XX^5$, can . . . ."

Researchers look up, nod coolly to the visitors, and go back to their mysterious manipulations at the chrome-plated control console. Is the modern lab's equipage really as important as stockholders, corporate officers, research directors, and technicians imply? No. The best equipped laboratory—the unit most likely to produce results—is organized around a group of men each of whom may have a great deal of hardware at his disposal, or none at all. Man is the basic research tool and the laboratory should be built around men, not staffed with machines to be operated by men.

People not only are the basic research tool, the individual technologist can be the *best* research tool, with or without accessory instrumentation. Whether a research man is the optimal operant in the lab depends not only upon individual ability, but also upon the quality of research management that directs his efforts. "Direct." "Manage." These words strike terror in the technologic breast, for they signify something less than reverent regard for the favorite sacred cow of science, freedom of inquiry.

With industry alone betting $6.5 billion on R & D, and with the nation sinking nearly 3% of its gross national product into scientific investigation, it is occurring to a number of highly placed people that this is a rather luxurious subsidy for what frequently is woolgathering or downright loafing.

No firm, even the largest and strongest, can afford totally random approaches to R & D. The cost of keeping a man in the lab is stretching

* Reprinted by permission from *Industrial Research*, Beverly Shores, Indiana. The article appeared in the issue of June, 1962.

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toward $50,000 a year. Few research tools have such high rentals. Most production tooling is cheaper.

Unfashionable Fabric

What, then, can be done to make better use of research talent? Most important, management must take a common sense approach to the nature of the creative process, unfettered by preconceptions of what constitutes management and science, and with a healthy sensitivity to the human dynamics—*including conflict*—which are the very fabric of individual or group creativity.

Some first steps toward this point of view are simple, but not painless.

Reject the fashionable ideas of how a research unit is put together, and consider the lab as a group of people *aided* by instruments, rather than the reverse.

Secondly, reject the platitude that research must be unfettered, free wheeling, and full of serendipity. Recognize the necessity of giving the research unit goals, direction, and speed, and then try to help them achieve that non-equilibriant condition.

Why get rid of preconceived notions of management? Because management training today is preoccupied with organic charts, the skeletons of industrial organization, dehumanized cadavers of corporate anatomy. The rest of the firm, in fact the very nature of the firm, usually is lost or ignored. There has been no mutation in the genes of goal-oriented industrial effort. Despite what many practitioners (and most professors of management) would like to believe, firms get things done, not by virtue of corporate organization charts alone, but by means of real and vital nervous systems made up of people.

Nervous Breakdown

Management of any group is difficult, but R & D management (with the possible exception of academic management) is the most difficult. Why? Because researchers are expected to produce innovation, or at least adaptation, rather than identities. By its very nature, research is the business of getting disorganized.

Subtle degradations in the creativity and competency of the lab staff are metamorphosed easily into higher product development and engineering costs, imbalanced product lines, inconsistent quality from model to model, and a long list of characteristics symptomatic of declining imagination and *esprit* in the laboratory.

Most R & D directors, conditioned by organization chart myopia, are ill-equipped to detect, assess, and correct breakdown in the organizational nervous system. Having been taught the sciences of management with

great emphasis on function, flow, and communication, research leaders seem overanxious to adopt new organization charts rather than supply the roots of good leadership. When things go wrong, the whole research group feels that the words of boxes on the current organization chart are at fault. Subordinates also seem to be seeking increased certainty of organizational structure (and stature). Man, being an uncertain quantity, is being written out of the management equation by mutual consent of management and managed.

But this is not a new story. For centuries man has attempted to replace personal arts with science, or quasi-science. It should be noted that this effort always has resulted in standardization, and standardization of output is the antithesis of what an R & D outfit is supposed to be doing.

Direction and Speed

Thus the quandary: how to give direction and speed to a group that is inherently unstable if it is performing at peak effectiveness.

How do you solve the quandary? You don't. Recognition of the fact that there isn't "an answer" is the hallmark of an effective leader. Experience demonstrates that the best leaders are eager to learn and utilize any new analytical technique, *as well* as possessing a deep sense of the art of leadership—a recognition of the essential ambivalence of men and organizations toward change and equilibrium.

Alchemy of Uncertainty

Engaged as they are in goal-oriented activity, in which they attempt to bring rationality to group endeavors, the best research directors have a healthy consciousness of the irrational side of man. The good leader indeed must remain poised in the midst of uncertainty, unfrustrated by the fact that men do not behave like automatons. Since R & D is an alchemy of uncertainty and change, the criteria for effective research direction are as binding as for any other form of leadership.

The skilled leader understands change and the dynamics of human growth, including that least discussed aspect of growth—conflict. Researchers, from the tallest ivory tower to the soldering bench, should recognize that growth hurts. While change *per se* does not necessarily mean growth, real growth does require change.

Several points of view, or philosophies of research direction, are corollary to the theorem that growth hurts. Since change is a requisite of growth, and since there are many paths to any goal, conflicts are bound to arise.

No management course, and no actual management position can school

researchers adequately in the unsubtle arts of dealing with conflict. Unfortunately, most management authorities avoid the subject altogether, or consider conflict in a most distorted light. For example, of five recent, respected texts on management, only one even mentions conflict, and then solely in terms of man-organization conflict.

Conflict ⇄ Authority

Similarly, authority as a concept is listed in just two of the volumes. None of these five books (all of them are expected to be basic texts for courses in management next fall), lists authority in terms of being a constructive, powerful management tool.

Granting that authority is a legitimate power that can be constructive or destructive, is it necessary to consider the concept in terms of social justification? Doesn't the question of social justification, so often raised, blur the study of skillful use of authority? In this "socially justified" frame of reference, students of management speak of constructive authority only when a management action is non-disruptive. Sometimes, in fact quite often, the *whole reason for a management action is to disrupt* an unsatisfactory set of conditions—conditions that by definition are harmful to the firm, even though its employees are most content with the status quo.

Many management authorities say power should not be used on pain of inhibiting individual performance. Then, pointing to the set of personality, educational, economic, and social factors found in the *laboratory* staff, research management is cautioned especially to use persuasion; to avoid conflict; to be Peacemaker General, rather than pacesetter.

Yet, today's supersensitivity about the use of authority is encouraging creative, competent, economical research, and development well may rest in blind misconceptions of what conflict is, where it comes from, and what good can come from it.

The fact that conflicts often produce good should not come as a surprise to the researcher. Conflicts are a sure index of difference, and something different is what the researcher is looking for. Conflicts in the research lab particularly are likely to be based on sets of facts and intuitions that can be viewed from several aspects and then resolved to produce important changes in the form of new products or processes. Conflicts among researchers also can be flags that mark anticipated problems in plans or projects. Thus, conflict avoidance, which is a distinct and detectable behavior pattern, also ignores potentially profitable points of view.

I am persuaded that the skillful leader understands conflict and practices the constructive use of power—not in the predepression robber-baron manner—but rather as *one* of the skills required to achieve human organizational growth.

If industry is going to get its R & D money's worth, if the revolution in laboratory equipment is to be implemented, and if our country is going to maintain its reputation as the world's greatest laboratory for the development of the human personality, we must recognize that man is our greatest research tool; that the pressure for new knowledge should include some common sense analysis of the human dynamics of the quest.

Chapter 25

THE OPTIMUM CLIMATE FOR INDUSTRIAL RESEARCH*

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Why is the research laboratory unique, and why are the scientists who work in it "different"?

What are the five basic components of a creative industrial research climate, and what can the manager do to develop them?

What management attitudes need altering for the sake of healthy relationships with the scientist?

Why is it that the manager must adapt his understanding more than the scientist does?

THESE ARE some of the questions I want to discuss in this article. My thesis can be stated simply, although its implications for management are not so simple and may even impress some executives as being radical. The idea is: *the creative work of research personnel is the basic product of the research laboratory*. And the degree of productivity evidenced by laboratory personnel will depend on the degree to which the optimum climate for professional research work has been developed and maintained by research management. Effective recruiting depends on such a climate; and problems of evaluation and compensation of technical personnel are simplified where this climate exists.

KINDS OF PEOPLE

It is difficult for a layman to get the feel of the working climate required by research personnel unless he understands the kind of people who work in the world of science and the influence on their values of the special training which made them scientists. There are many differences between the ideal working atmosphere of a research laboratory and that expected, or even desirable, in the other parts of an industrial organization. The particular values about themselves and their work that researchers bring to their jobs are atypical, in the sense that they are by and large strange to the business world.

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In seeking an understanding of these differences, an important clue can be found in the universities, where most research people work for a number of years while earning advanced degrees in their special fields. Indeed, it is fair to state that, though individual researchers are frequently hired from the laboratory of another company, the environment that has had most to do with their way of thinking about themselves and their work is the university laboratory in which they learned about science and scientific methodology.

Head in the Clouds?

The industrial community tends to look on the academic environment and the academicians who inhabit it with a curious mixture of suspicious derision and amused tolerance for "absent-minded professors" and "head-in-the-clouds thinking." Even though the men concerned may be renowned in their special fields and, in the case of scientists, often paid considerable sums as consultants by industry, it is a commonplace to hear industrial managers say something like, "These boys are fine if they stay in their own back yard where they're carefully sheltered from the realities of life. But they're pretty helpless when they get into business—they just don't understand what goes on."

Considering this assumption, it is not hard to understand why industrial managers find it difficult to deal with the people who work in their research laboratories. Yet a sophisticated understanding of these people and their values is a necessity if the optimum climate for creative research is to exist in a nonacademic laboratory.

What is it about the academic laboratory that makes it a unique institution? What is it the scientist finds there that influences him throughout the rest of his life?

THE UNIVERSITY LAB

The atmosphere of the university laboratory is a curious mixture. On the one hand, we have the permissiveness, lack of pressure, and concentration on the ideas and knowledge characteristic of academic life, and, on the other hand, the rigorous discipline of scientific methodology. And though laboratories vary in many ways—their reputation, their particular areas of interest, their ability to attract financial support, the equipment and facilities available, and so forth—they always have certain characteristics in common:

1. Perhaps the single most important characteristic of the university laboratory as implied above is the *emphasis on basic research and on the discipline of scientific methodology*. This implies the pursuit of knowledge according to a rigorously defined methodology which demands acute observation, separation of facts from opinions, meticulous recording of results, and the reporting of verifiable conclusions.

It is no accident that most of the basic research done in this country, with the exception of a few laboratories supported by our largest industrial companies, is done in universities. The requirements for a PH.D. degree in the basic sciences include original research in the special field of the investigator. Furthermore, the goal of any university is the pursuit of knowledge without reference to the practicability of its application. As a result, the chemist, physicist, or metallurgist seeking his degree usually chooses a thesis subject which will contribute a small slice to the sum of basic knowledge in this field and which will demonstrate, as he seeks this knowledge, his expert use of the scientific methodology which graduate programs in the sciences are designed to teach him.

2. A second fundamental characteristic is evidenced by the *permissive, low-pressure atmosphere* which is found in the university laboratory in common with other academic units. Within this atmosphere individuals make many decisions for themselves about their work and the direction it will take, while at the same time always having the counsel and direction of the faculty group available to them. Within such a climate graduate students learn to think for themselves and necessarily become accustomed to an independence of thought and action which is difficult to eradicate once it becomes a part of their professional life.

3. Another characteristic of the university laboratory is found in the *opportunity for constant interaction with colleagues and faculty who are interested in the student's own and related fields of investigation.* The student becomes used to being able to talk about his problems with sympathetic and interested co-workers who have enough knowledge of his field to ask stimulating questions and suggest interesting alternatives. He discovers that his own ideas are often sparked by such conversations and soon learns to depend on the availability of interested and informed colleagues.

4. A further characteristic concerns the basic motivation of faculty and students alike. This motivation can best be described as *a desire for intellectual development and achievement.* The heroes of the scientific world are men who have made great discoveries, men who have added significantly to the world's knowledge in various fields. The graduate student of science, no matter what his future career may be, never quite gives up the hope that he too will someday record significant additions to our store of fundamental knowledge.

5. A final important characteristic is suggested by those already stated, and it concerns the way scientifically oriented people evaluate themselves and their colleagues. *Scientists are evaluated according to their knowledge of their field, the degree of expert methodology they display in their work, and the originality of thought and method evidenced in reports of their work.* For the rest of their careers they will always be evaluated in these terms, and, since no one else knows enough about their work to evaluate it accurately, the judgment of their colleagues in the scientific world will always be more important to them than judgments made by persons outside this area.

SCIENTISTS AS INDIVIDUALS

Now let us consider research-oriented personnel as individuals. The scientist who emerges from the environment of the university laboratory thinks and behaves as he does because of the influence of this environment, or maybe he entered the academic world in the first place because he thinks and behaves as he does; it hardly matters. Whether you believe the chicken or the egg came first, the resultant personality matrix of the

scientist is familiar to anyone who has associated at all closely with these men. Men of science differ as individuals from each other, of course, but they differ even more significantly—typically and in general—from other people.

Starting out with a high degree of intelligence, they have been subjected to an intensive and lengthy education from which they have emerged as specialists and professionals in highly complex fields. Perhaps because of the demands made on men intellectually in learning all there is to know about a particular area of technical knowledge, their field of interest tends to be highly concentrated. In thinking about things, people, and events, they tend to be logical, opinionated, impatient, intense, thorough, meticulous, reserved, and clannish.

More often than not, their independent ways of thinking and expressing their thoughts, along with their impatience with abstractions and intuition tend to separate them from other people and to inhibit the outgoing characteristics of their personality. They normally regard conformity as a cardinal sin, and in their efforts to avoid it they often behave in unexpected ways or become interested in the bizarre and unusual.

Special Work Pattern

Careers in the world of science tend to follow a pattern in the years immediately after the scientists have earned their doctorates and as they gain experience and confidence and delve deeply into the intricacies of their special fields. These years and those until they are about 45 are their best creative years.

Few men of science who have not made their reputations by the age of 40 will ever be regarded as at the top of their field. In their late 40's and 50's they begin to slow down, lose their fine edge, and fall behind the advances made in their field. They often retain an intensity and dogged curiosity plus a wealth of experience which enables them to spark and teach younger men, and of course there are those who are able to maintain a high degree of creative effort throughout their working lives. But the average scientist is like the rest of us. He slows down as he grows older. He knows too many things that will not work to be really creative.

The combination of the unusual personal characteristics of most scientists and their knowledge that their golden years careerwise are relatively few often results in behavior which is misunderstood outside the environment which produced it. When they move from the academic to the industrial world, the fact that they think and behave differently makes life difficult for practically everyone in an industrial organization who must deal with them. Businessmen find it hard to understand almost everything such men say and do. They do not understand why scientists who have chosen to work in nonuniversity laboratories cannot conform to the traditional ways of doing things which are built into the organizations

supporting such laboratories. They cry, "These men are not at the university any more. They are in business."

Managers expect the demands of the organization to be met by the people working in the research laboratory to the same measure and in the same way these demands are met by other departments of their company. They do not believe that such demands are unreasonable, but they do believe that the counterdemands made by scientists trying to do research in the industrial environment reflect the desire of these men to be coddled, protected, and set apart from the rest of the organization.

Lacking understanding of the research process and the research mind and personality, industrial managers are inclined to meet these demands half-way or not at all—and the climate for research in their organization suffers accordingly.

Balking at Organization

While what I have said applies most specifically to the research scientist, there is much in common between these men and all professionally trained technical people. Professional training in itself, whether it be in medicine, chemistry, or engineering, appears to predispose those who go through it to unhappiness or rebellion when faced with the administrative process as it exists in most organizations. Scientists and engineers *cannot* or *will not* (and is there any practical difference?) operate at the peak of their creative potential in an atmosphere that puts pressures on them to conform to organizational requirements which they do not understand or believe necessary.

The tendency has been for industrial managers to insist that the scientists working in their laboratories recognize as a truism the often quoted "When in Rome. . . ." They point out that a laboratory supported by the funds of a profit-making organization exists for different reasons and must be run differently than a university laboratory. I have known of industrial research organizations where this belief is carried so far that all workers, including senior scientists, are required to punch a time clock on entering and leaving the laboratory!

This practice is an almost ludicrous example of the extent to which lack of understanding can wreck morale. Even more devastating, however, because they are less obvious, are the subtler attempts of industrial managers to run their laboratories in the same way they run a factory which produces automobiles or soap.

CLIMATE FOR CREATIVITY

Since the dawn of history the professional in any field has been used to and expects a certain deference based on his advanced knowledge and skill. The man of ideas, be he artist, philosopher, or scientist, has always

insisted on creating for himself a climate where ideas flow freely or, at the very least, where he can find independence within a group effort dedicated to similar pursuits.

Creative activity cannot be forced. The creative people of this world have always been the free, unchanneled minds, the nonconformists, the individualists, the uninhibited. It is perhaps worth noting that throughout history the greatest additions to our cultural heritage have been made where and when the artist has been revered. It is perhaps even more to the point to note that in Russia, where the scientist is placed on a pedestal, scientific and technical progress is racing ahead today at a pace we are finding difficult to match.

In the Western world the tendency has been to defer to the scientist only, as it were, after the fact—after he has become great, after he has made important contributions—rather than while he is struggling to make them. This is, of course, not always so, but the exceptions are illuminating. Thus:

> We can recall the amusing, touching, and fascinating story of Irving Langmuir, who, in the early years of the General Electric Laboratory, almost helpless in affairs of everyday life, was surrounded and supported by people whose major responsibility was to smooth the way for him and to see to it that he wanted for nothing, either personally or professionally. At the end of his career, speaking at a testimonial banquet in his honor and trying to explain his productive life at the laboratory, he said, "You know, I never had to worry about budgets."

Little Tin Gods?

We do not, of course, want the scientists and other professional technical people working in industrial laboratories to become little tin gods of the industrial world. But it is highly desirable that we learn to create a climate which will provide such men, while they are young and relatively unsung, with the basic atmosphere necessary for the fullest emergence of their creative potential.

If we assume, as we must, that the basic product of the research laboratory is the creativity of research personnel, then the problem seems hardly different from that facing the plant manager who attempts to produce as much of that plant's product as possible with the men, materials, and money available to him. The plant manager knows that he must try to understand the motivations of his work force if he is going to stimulate productivity successfully. It appears obvious that those responsible for the management of research must do the same thing. They must learn to understand the professional researcher and, having succeeded in this, must then work to create the best possible climate for creative research work.

The optimum climate for creative research can be thought of as being made up of several separate elements which blend together to form an

atmosphere within which professional workers can discharge their creative potential with a minimum of distraction or irritation. Most of these elements follow naturally from the facts previously mentioned about the kinds of people working in research and the values they prize as a result of academic training. Yet the make-up of a good climate is always an elusive thing; certainly it has perplexed many businessmen thus far in our history. Accordingly, I have singled out what might be considered the five most basic elements of the climate for creative research. Let us look at them in some detail.

RECOGNITION OF STATUS

As mentioned previously, few companies in the industrial world have found it easy to assimilate their professional technical employees. Generally speaking, they have tended to assume that the research, engineering, or other technical components of their organizations should be organized and administered in the same fashion as the rest of the company. In acting on such assumptions industrial management has failed to recognize the importance to research workers of their professional status. They have chosen to ignore or to find "queer" or "unrealistic" the needs expressed by research workers for *different* organizational structures and *new ways* of administering research organizations.

While most research workers are unusually intelligent and highly individualistic people who would prefer to work for and by themselves, they actually are about the only professional people who cannot practice their profession without some kind of institutional support. When they decide to come to work in industry, they do so, not because they like the idea of working for an industrial organization, but because:

They need the facilities, equipment, and project opportunities that they cannot finance for themselves.

They do not want to have to worry about running a business.

They need the stimulation of colleagues active in the same or allied fields.

They desire or need the somewhat larger salaries obtainable in industry in comparison with those typical of academic institutions.

No wonder many industrial managers feel that in return for the facilities, service, equipment, and larger salaries provided by industry, any scientist should be willing to conform to the requirements of the industrial organization. They cannot help resenting the fact that he appears to want to have his cake and eat it too. One can certainly sympathize with such feelings, but I have found that action *based* on them is almost guaranteed to destroy or seriously limit the creative potential of a research group.

Assuming that the manager recognizes the dangers of insisting on conformance from research personnel and wishes to signal his recognition of their professional status, how exactly can he translate thought into

practice? Four policies or rules of action impress me as being especially helpful.

1. *The manager should refuse to impose rules and regulations on the professional staff which imply their lack of intelligence, maturity, or understanding.*

Most industrial organizations have found it necessary to promulgate such rules and regulations as a way of controlling the activities of the *rest* of their work force. A typical example concerns hours of work:

> Most companies specify reporting and quitting times, strictly enforce overtime regulations, and, as noted previously, make use of time clocks for both manual and clerical workers. Even where companies have insisted that research personnel punch time clocks just like every one else, managers of other company departments often protest the fact that scientists insist on coming to work at noon and working until midnight whenever they feel like it, instead of working normal hours. They are particularly apt to protest (and with some justification) if the research organization is housed within or close to company plants or offices where employees who observe more traditional hours of work can observe the goings on at the research laboratory.

Recognizing the ill grace with which professional workers take to organizational discipline, many companies have decided that the only way to handle the situation is to remove the research activity physically from the rest of the company, and certainly this is often a workable solution to the problem. If it is not workable, for financial or other reasons, then management must balance the disadvantage of annoying manual and clerical workers against the disadvantage of annoying scientists. Somewhere along the line a cost must be paid.

2. *The manager should insist on consulting with professional workers about plans or proposals which will affect them and/or their work before decisions are made.*

The highly skilled professional worker does not want to be bothered with the details of administration, but he does want to have a chance to express himself concerning policies, plans, or proposals which will directly affect him.

There are numerous ways of doing this, and the desired result will be accomplished if the manager's willingness to consult is sincere and based on real understanding of the need for it. It is rarely necessary to communicate explicitly with every member of the technical staff concerning matters of this sort. Usually formal or informal communication with senior members who can represent the rest of the technical staff is sufficient. Depending on the size and structure of the laboratory organization, communication on these matters may be through formal meetings or informal individual conversations.

No matter how such communication is accomplished, it is important to remember that the manager should never consult with the attitude that

plans are fixed and that he is merely letting the men in the organization know about them before they become operative. On the contrary, his assumption is that the ideas of men at the bench could and often do cause him to change or at least modify his own ideas about how things need to be done.

It should be pointed out that if the research manager is as closely in touch with the scientists as he should be, he will already know how they will react to various proposals involving them, and he will be able to act accordingly without specific consultation. He ought to be very sure of his ground before attempting this, however. Even if he thinks he knows, it never hurts to ask a few questions.

3. *Within reasonable limits imposed by the business situation, the manager should allow researchers considerable freedom to plan their own work.*

This is perhaps the most controversial suggestion made so far. In many ways it goes against all of the logic of management and impinges on prerogatives which industrial managers usually insist are theirs as managers.

Certainly the need to control expenditures, to budget, and to plan for optimum results from money spent have always been management functions. I am not suggesting that managers of research, or top management, should turn these functions over to scientists at the bench. I *am* saying that scientists at the bench know better than anyone else the areas of technical activity which, if worked on, would produce the desired results. Particularly in regard to basic research, they know best how much work should be done to bring projects to a conclusion.

If a company is not willing to support effort in a researcher's field of interest, the researcher should not be working in the company's laboratory. If a man is hired to do certain work, he is presumably the best man who can be found in his field. This being so, the company should be willing to support him and his work to a degree which has been agreed on in substance before he comes to work in the laboratory.

Needless to say, conditions change—competitively, financially, or in some other way—and long-term projects may have to be revised downward in accordance with these changes. Also, a company cannot always support all the work in a particular field it would like to, and it is unreasonable to expect unlimited support for any but the most vital projects. Limits do have to be imposed—limits which are a product of the total funds available, the caliber of personnel in various fields working at a particular laboratory, the time available for completion of important projects, and other less important factors. The manager cannot and should not expect bench scientists to make final decisions in the planning and control areas. But he can and should give considerable weight while making these decisions to the ideas expressed by his men concerning the work which should be done in their fields.

In this respect a research organization differs considerably from the other parts of a company. The manager of a production organization is supposed to know more about the operation of that organization than anyone else; that is one of the reasons he was made a manager. In research laboratories this is not true. A manager cannot possibly know as much about several complex technical fields as the men who specialize in these fields. Necessarily, therefore, he has to trust them to make realistic proposals concerning future requirements within the budgetary limits imposed on the laboratory as a whole. To this end he can make things easier for himself if he consciously makes a practice of keeping the men at the bench informed about the company situation.

4. *The manager should encourage (but not press) scientific personnel to keep up with their fields and to add to their professional stature by attending meetings of professional societies and writing and delivering technical papers.*

The important distinction here is between encouraging and exerting pressure. Because the over-all standing of a laboratory in the eyes of the scientific world increases in almost direct relation to the number of technical papers presented by laboratory personnel, research managers often are under a natural temptation to prod their personnel to write up and publish the results of their research activities. Certainly it is important both from the point of view of individual scientists and from the point of view of the laboratory itself that these papers find their way into scientific journals. But the need for publication often preoccupies a research manager to such an extent that he places undue emphasis on it.

Certainly, as in the academic world, the ability to do work worthy of publication and the ability to write up results in publishable form is a prerequisite if a scientist is to gain respect and the financial and other benefits which go with an increase in professional stature. The manager of research should make it clear to scientific personnel that this is so. Having made this clear, however, he should tread delicately. There is a difference between making the time available for men to write up their results and insisting that these results be written up when a scientist is preoccupied with furthering subsequent projects.

FACILITIES AND ASSIGNMENTS

The second basic element of the climate for creative research involves the need for:

1. First-class facilities for whatever work is to be done in the laboratory.
2. Colleagues of high professional stature.
3. Work assignments of real interest to scientists on the professional staff.

The need for these things necessarily imposes on research management the requirement of careful planning. Many research projects have floun-

dered and died because men were hired to work on them when funds were not immediately available to provide the equipment and facilities required. Many projects have been slowed considerably because the scientists involved were unable to interact with colleagues who could spark their imagination and ask the questions which so often lead to breakthroughs on apparently insoluble problems. Many laboratories have lost good men who were not provided with assignments of real interest to them in their special fields.

I realize, of course, that the realities of industrial life do not make it easy for the research manager to provide in full measure the requisites mentioned above. Given a critical problem in an isolated field of technical work, he often has to hire a good man, put him in a corner with the minimum of facilities, and hope for the best. Given good men in a variety of fields, he often finds it impossible to keep them continuously supplied with critical problems that are of real interest to them. Nonetheless, constant effort should be maintained to give the scientists what they need. If a company is not prepared to do this, it should reappraise the situation before it goes about hiring researchers.

RELATION TO ADMINISTRATORS

No scientist wants to be regarded as just "an employee" (even a high-class one). As mentioned earlier, he typically does not really want to work for an organization. The fact that scientists, generally speaking, *must* work for an organization does not make them any the less conscious of their status as professional people. They regard themselves as "entrepreneurs of ideas"—as creative people who require a very special environment if their creativity is to emerge. As such, they believe that they should be served by the organization for which they work, rather than be regulated by it.

More specifically, scientists do not like to think of those men who have chosen to pursue administrative careers in research as their superiors. Regardless of the fact that in an industrial society managers are regarded as superior to the operator or worker, scientific personnel would prefer to regard these men simply as individuals who have chosen to pursue a career in the administration of research rather than a career at the bench.

Troublesome Assumption

It is in this area of relationships that the atmosphere or climate for creative research may be most easily solidified or destroyed. If the manager of research chooses to regard himself as a manager with all the prerogatives of management, and as a superior with all the ways of behaving implied by superiority, he will find it difficult to create the climate I speak of, no matter how carefully he works to provide the other vital elements of it.

On the other hand, he will find it very difficult to regard himself *as a superior* if his attitude toward his operating colleagues is such that he is able to provide in full measure the other elements of the creative climate. To feel superior, one must believe that he is better at what he is doing than anyone else involved in the activity. While the manager of research may be better at getting things done through people than some of his colleagues at the bench, he cannot realistically assume that he knows enough to be better than most of them at the work they are doing.

Just as a doctor's income is usually considerably higher than the income of the man who administers the affairs of a hospital, the income of a creative scientist may, and often should be, higher than that of the man who manages the laboratory where he works. It is very difficult for men who have been brought up in industrial organizations to understand this radically different point of view. They are steeped in the assumption of our industrial society that the function of management is more important than, and should be compensated more highly than, any work done which does not involve responsibility, authority, and the supervision of the work of others.

The fact remains: the point of view of the scientist, influenced by his years in the university laboratory, is a stubborn reality which, if ignored, will invariably result in a loss of creative potential for the laboratory as a whole.

OPPORTUNITY FOR MOBILITY

If we think of research work as involving a spectrum with basic research at one end and product development at the other, we find it important to organize our laboratories and plan work along the spectrum in such a way that research personnel can move from basic research through applied research to development work as and if their orientation and interests change.

Usually, a young scientist coming to work in industry for the first time would like to continue working in the basic research area which engrossed him while he was at the university. Having already had to reorient his thinking when making the shift from academic to industrial life, he finds it far easier to make the shift if the initial projects on which he is to work are closely allied to the work he was doing before this change in environment.

Unfortunately, many industrial laboratories do not support work at this end of the spectrum. At best, the research they are doing may be termed "applied." This being so, it is of course impossible to assign new men to fundamental research. But in those industrial laboratories large enough to support fundamental research, it has been found very helpful to assign new men to work at the fundamental end of the spectrum so as to give them time to look around and acquaint themselves with the other

work being done in the laboratory. This gives the man an opportunity to decide in his own good time and with reference to his performance in comparison with his colleagues' whether or not he would like to remain in fundamental research or move along the spectrum to applied research, or even to development work.

If often happens that a man will be doing work in the fundamental area which results in some sort of application. Such men often decide they would like to follow this work into the applied area rather than turn to other projects of a fundamental nature. This follow-up possibility is one great advantage which industry has to offer.

Choice of Work

There appear to be two major factors which influence a man's decision about the kind of work he wants to do:

1. He is constantly evaluating himself as a scientist in comparison with his colleagues. If he finds that his performance in the fundamental area does not stack up with that of other men at the laboratory, he will often consider work at other points along the spectrum.
2. The other factor involves his personality. If he finds that he enjoys dealing with people in connection with projects on which he may be working, he will be more inclined to move along the spectrum toward development work, where interaction with people from other parts of the company is an important element in the successful completion of work.

The main point to be remembered is that the graduate scientist is unfamiliar with the rewards offered at various points along the spectrum until he has had a chance to accumulate some insights concerning the advantages and disadvantages of particular kinds of work in the industrial environment. Many people concerned with research in industry decry the fact that all young scientists seem to want to do fundamental research. This is heard most often in laboratories where no basic research is done. Managers of such laboratories must recognize the realities of their situation and understand that it is hardly unusual for anyone to want to do the kind of work he has been doing rather than to shift suddenly from one kind to another.

Where no fundamental research is done, this should be made quite plain during the recruiting conversations. It is not unknown for recruiters to dangle the carrot of fundamental research before the noses of graduates, and hope that the new men will quickly adjust to the level of work actually being done in their laboratories. This recruiting technique is not apt to improve the climate for research and often results in such disillusionment that the new man is never able to integrate his preconceptions with the facts, and he soon departs for greener pastures.

A laboratory can be organized and work can be planned so that men can shift jobs quite easily as their interests change and as they become more familiar with the kinds of work being done. In many cases such

changes of interest will be obvious to all concerned, and appropriate transfers can be made. In other cases a man may not be aware of his potential in other areas until the possibilities are suggested to him. It should be emphasized, however, that there is a fine line to be drawn between such suggestions and attempts to force men to make such changes because it is more convenient for management.

IMPORTANCE OF RESEARCH

It seems hardly necessary to mention that the final factor in an optimum climate for creative research is management's recognition of the importance of the research effort. But since there are so many companies where this recognition is not apparent, its importance must be stressed.

Certainly it is difficult for top management of companies whose research effort is relatively new to have complete faith in that effort until results are forthcoming, but unfortunately results are often a long time coming and companies that have decided to support a research effort have made a relatively expensive decision. It is perhaps unfortunate that early management reviews of such decisions sometimes take on the aspect of criticism and lack of faith in research management and personnel, with consequent loss of morale. That this is so merely underlines the necessity for quickly establishing and maintaining over the long run an atmosphere of confidence. Once this atmosphere is established, periodic reviews of research policies and financial commitments may be undertaken as necessary without endangering the creative climate.

OTHER PROBLEMS

I have left until last—and for quite brief treatment, at that—any discussion of the problems of recruiting, evaluation, and compensation. These problems are the subject of much attention and worry, yet the most important things I can say about them I have already said. Almost everything depends, it seems to me, on the climate in the laboratory. If the climate is right, the problems either disappear or become much easier to handle; but if the climate is wrong, the problems loom up despite the most carefully devised methods and techniques for alleviating them.

Recruiting

Take recruiting, for example. It is considered to be one of the most widespread problems in industrial research, yet recruiting practices *as such* are not, in my opinion, the issue at all. The real clue to the trouble would seem to lie in the fact that the list of industrial laboratories where a favorable climate for creative research is known to exist is discouragingly small.

I would venture a guess that a survey of graduate students who are about to take their advanced degrees asking them to name the industrial laboratory they would prefer would result in first choices being limited to four or five laboratories in 90 percent of the responses. Each year these laboratories get the cream of the crop from the universities. Starting salaries are no higher; in fact, they are often lower than those paid by laboratories that do not have such enviable reputations. The word gets around, and the judgments about various laboratories fed into the scientific grapevine can have devastating effect on a particular company's recruiting problems.

There is, of course, some public relations involved in establishing a good reputation for a particular laboratory, but in the scientific and engineering world public relations will not help a company for long if the creative climate does not exist. Furthermore, public relations for a laboratory cannot be laid on whenever you start a recruiting drive. It is a continuing effort backed up by the ability to tell the story scientists want to hear. If management wants to go to the expense of inviting prospective recruits to visit the laboratory, or if it is eager to maintain close relationships with the university professors who know the prospects as students, that is fine and good; these are very worthwhile practices indeed. But they can only help. They cannot do the main job.

Making Appraisals

Much the same thing can be said about the problems of evaluating, compensating, and developing the competence of research people. In a poor climate, all kinds of disagreements, failures in communication, and bad feelings arise. They can frustrate all management methods. Yet the same methods will be successful, and frustration will rarely occur, in a good climate. This is so mainly because the members of the technical staff know that their past, present, and future are being evaluated and rewarded by their colleagues in the world of science—not by "management." The setting is therefore a conducive one for efforts to:

Establish criteria that discriminate realistically between men who are merely satisfactory in their technical fields and men who are known (by their colleagues) to be outstanding.

Make performance appraisals that reflect not only the section head's judgment but also the judgment of other technical men who know the researcher well.

Set salary ranges that reflect current market practice.

Emphasize a man's future possibilities and development in periodic discussions with him of his performance.

CONCLUSION

The problems of status, security, and discipline which plague those laboratories lacking an atmosphere of confidence and mutual respect

between scientists and management simply do not exist in laboratories where this climate is present. The issue is largely a management one. The essential element for creation of a good climate is a belief on the part of management that the basic product of a research laboratory is the creativity of research personnel, plus a willingness among managers to revise their traditional attitudes so that their behavior while interacting with scientists will reflect this belief.

Does such a conclusion place too much weight on the need for changes in traditional management attitudes? Is it based too much on the assumption that the scientist should be allowed to maintain his values without change? Essentially, I *am* suggesting that management attitudes should change if they are not based on understanding of and respect for the values of scientific personnel. All of my own experience underlines the often stated fact that you cannot try to change professionals into "organization men" if you expect them to be creative. William H. Whyte, Jr. has made this point in no uncertain terms in an important chapter of *The Organization Man.* Among other things, he has this to say:

> Management has tried to adjust the scientist to the organization rather than the organization to the scientist. It can do this with the mediocre and still have a harmonious group. It cannot do it with the brilliant; only freedom will make them harmonious.[1]

At another point Whyte observes:

> A company cannot bring in young men and spend several years trying to make them into one kind of person and then expect them, on signal, to be another kind. Cram courses in "brainstorming" and applied creativity won't change them. If the company indoctrinates them in the bureaucratic skills and asks them to keep their minds on the practical, it cannot suddenly stage a sort of creative play period and then, on signal, expect them to be like somebody else.[2]

So the point of view implied by my concept of the optimum climate for research *is* heavily weighted on the side of a need for basic changes in management attitudes. I believe strongly that managers must learn to think about and behave toward the professional employee "differently." If they seek the maximum return of their investment in research, these changes in attitude are also in their own self-interest.

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[1] (New York: Simon and Schuster, Inc., 1956), p. 213.

[2] *Ibid.*, p. 215.

Chapter 26

NINE DILEMMAS IN INDUSTRIAL RESEARCH*

HERBERT A. SHEPARD†

PROBABLY no other branch of industrial activity causes as much managerial unrest and uncertainty as does research and development. The concern is due largely to the enormous difficulty of evaluating the laboratory's actual and potential economic contribution to the firm. The inapplicability of traditional methods of calculation creates an interest in discovering other ways of judging whether performance is good or poor. Through this door the never-never land of opinion, myth, and prejudice is entered—a land which breeds unrest in manager and scientist alike.

It used to be said that the way to do industrial research was to hire good scientists and leave them alone. Certainly no such simple formula can be taken seriously today. But neither have we arrived at the point where discovery of the secrets of successful research and development management can be claimed. Perhaps the best that can be done at present is to impose some order in the never-never land—to identify some of the problems that make for unrest and some of the issues that need to be resolved.

THE LABORATORY: DEPARTMENT OF TODAY OR DEPARTMENT OF TOMORROW?

The needs of the firm as interpreted by top management exert a powerful influence on the laboratory. For public relations purposes—and it is generally held that a number of laboratories have been founded primarily for the sake of institutional advertising—the laboratory is likely to be referred to as the "Department of Tomorrow." In many companies, however, "today" is the principal concern of the laboratory, which may be quite incapable of bringing about a different tomorrow. The potential of the laboratory may not be understood, because of inadequate represen-

* Reprinted by permission from *Administrative Science Quarterly*, December, 1956, pp. 295–309.

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tation in top management. Even where it is well represented, its scope is likely to be limited to questions of technological feasibility; hence its power as an innovative force is less than it might be.

While certain kinds of new products and processes are undoubtedly desired by most companies engaged in research and development, other requirements are likely to have high priority. Many, and in some cases nearly all, of the laboratory's activities contribute relatively little to the development of new products and processes. For example, technical assistance and "trouble-shooting" in other parts of the company take up much laboratory time. Some laboratories are required to supervise quality control activities. Defensive research must be carried on; i.e., limited resources must be devoted to keeping abreast of certain fields, not with a view to innovation, but to ensure that the company would not be far behind its competitors should one of them take an innovative step. Similarly, much effort may be devoted to imitative and substitutive work, to match competitors' products. Patents may be produced for trading purposes only. In industries where technology is complex, the laboratory may be regarded as a training ground for managers and specialists needed in other parts of the company.

Thus in industries which are highly competitive, where no firm has a large cushion of monopoly, the laboratory is needed to meet immediate competitive threats, and few resources are available for expensive innovation or long-term research. The consequences for the scientific staff of the laboratory are likely to be frustration and disillusionment.[1]

A "research-minded" top management in a company able to make the high-risk investment involved in long-term research and development provides a different environment for the scientific staff. Many companies have established research centers with campuslike surroundings and an "academic" atmosphere. The term "research-minded" is a common one in laboratories and seems to refer not only to an interest in taking a risk of this kind, and hence a willingness to make a long-term investment, but also to an understanding of and sympathy with the needs and desires of research and development personnel. Sometimes the term seems to mean an interest in research for its own sake.

Many other sets of managerial expectations also have an influence on the laboratory. Company policies and procedures, beliefs about proper methods of organization, stereotypes about research and scientists, and comparisons between the conduct of laboratory personnel and other company personnel draw certain patterns of response from the research

[1] Warren G. Bennis, "Role Conflict and Market Structure in Two Physical Science Research Laboratories" (unpublished manuscript, Massachusetts Institute of Technology, 1953). See also Bennis, The Effect on Academic Goods of Their Market, *American Journal of Sociology*, 62 (July, 1956), pp. 28–33.

and development staff. All such outside forces influence the internal structure of the laboratory organization and affect the attitudes and activities of research personnel and the contribution of the laboratory to the firm.

THE SCIENTIST: LOCAL OR COSMOPOLITAN?[2]

Company policies and practices are usually intended to ensure a prior interest in the company's welfare on the part of every employee, to lead him to identify his own success with the company's. Salaries, fringe benefits, personnel programs, and the implied threat of punishment for disloyalty are employed to this end. The scientist may, however, identify himself primarily with his professional group. Professional and scientific values emphasize the importance of contributing to knowledge, and prestige is granted on this basis. The scientist is supposed to be guided by intellectual curiosity. His motto is, "How much do we know about this?" whereas the businessman's motto is, "What is the value of this to the company?"

These conflicting forces are disruptive not only of laboratory-company relations, but also of relations within the laboratory. The research staff itself is likely to be divided into what Robert Merton calls the "cosmopolitans" and the "locals."[3] The former are oriented toward success as members of their profession, and their interest in the company is limited to its adequacy as a provider of facilities for them to pursue their professional work. Since they are productive, they may be valuable to the company, but such value is an almost accidental by-product of their work. The locals are good company men, but their interest is likely to be less in their work than in their advancement in the company. They may therefore be of less value to the company than the cosmopolitans. This state of affairs creates a dilemma for management. The locals are likely to be more observant of company policies and procedures, create fewer "human relations problems," and in all disciplinary respects be good employees. The cosmopolitan is more likely to be a "problem person," without whom the organization would function more smoothly.

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[2] The following four sections draw upon a number of sources, principally Lowell W. Steele, "Problems of Integrating Scientific Research and Industry" (unpublished doctoral thesis, Massachusetts Institute of Technology, 1952); Warren G. Bennis, "The Social Science Research Organization: A Study of the Institutional Practices and Values in Interdisciplinary Research" (unpublished doctoral thesis, Massachusetts Institute of Technology, 1955); and R. K. Merton, *Mass Persuasion* (New York, 1946).

[3] Robert Merton, "Patterns of Influence: A Study of Interpersonal Influence of Communications Behavior in a Local Community," in Paul F. Lazarsfeld and Frank N. Stanton, *Communications Research, 1948–1949* (New York, 1949). Pelz and associates use the terms "institution-oriented" and "science-oriented" to make a very similar distinction. See Donald C. Pelz, Glen D. Mellinger, and Robert C. Davis, *Human Relations in a Research Organization*, I and II (Ann Arbor, Mich., 1953).

THE RESULTS OF RESEARCH: GUARDED SECRETS OR ADVERTISED ACHIEVEMENTS?

The company's interest in a research program depends in part on the prospect of profit resulting from the lag between itself and its competitors in the introduction of a new product or on the prospect of a more permanent monopoly. In companies which owe their past success in part to the careful protection of "trade secrets," there is usually a desire to keep the laboratory activities under wraps. The scientist's professional standing depends, however, on publication of his achievements. Even where secrecy is not the issue, management may deplore the cost in professional time and laboratory expense required to prepare material for professional publication or for presentation at meetings of professional societies. Despite C. F. Kettering's dictum to the effect that locking the laboratory door excludes more than it keeps in, and despite the importance of establishing a good scientific reputation for the laboratory as a recruiting device, relatively few managements actively encourage publication of research results.

THE BUDGET: COMPANY TIME OR RESEARCH TIME?

Companies operate on annual budgets, but research and development projects are likely to go on for years without producing useful results. It may take a long time to investigate a research problem even to the point of estimating the probabilities of eventual solution, and this is a source of strain between scientist and businessman. Management is frustrated by its inability to determine whether progress on a project is as good as could be expected; the research staff is frustrated by management's inability to understand the nature of the technical problem. This communication gap can lead to demoralization of the research staff and to a loss of confidence on the part of management. Or it may lead to more complicated states of affairs. For example, the laboratory may report only on those projects in which it can show the kind of progress that management understands, other research activities being "smuggled."

In another way as well, difference in time perspective produces conflict. To the company, a new competitive threat, a delay in production, a dissatisfied customer are problems demanding immediate attention. To the research worker with his heart in a long-term project, such problems are irritating interruptions, and if he is frequently called upon to "put out fires" he is likely to become resentful and suspect the sincerity of management's support of his work.

AUTHORITY: DELEGATED OR SHARED?

Two organizational traditions, the professional and the industrial, meet in the laboratory. Internal organization of the laboratory will be discussed

in greater detail below, but certain conflicts between the two traditions can be pointed out here. Industry respects certain kinds of orderliness in productive organization, and takes them as evidence of efficiency. Thus centralizing services, controlling hours of work, budgeting time, controlling expenditures and decision making by graduated delegation of authority from top to bottom, and many other practices are regarded as elementary principles of good organization.

The scientific and professional organizational traditions are based on assumptions that are different from those of industry in some respects. In the first place, power is supposed to be exercised not from top to bottom, but sideways. Achievement is evaluated by "the weight of scientific opinion." Unless a colleague's competence or honesty becomes suspect, he is expected to make all decisions relating to his work for himself. The idea of a boss is an anathema, as are the other external controls imposed by industrial methods of organization—in fact, they are held to be inconsistent with the basic tenets of professionalism. "Professional people work as 'senior' and 'junior' rather than as 'boss' and 'subordinate' . . . the professional thinks of [a project] as a group project, with each member of the team having independent responsibility vested in him."[4]

Most laboratory managements make certain gestures in recognition of the scientist's "rights" as a professional. For example, time clocks may be omitted, or the scientist may be allowed to incur expenditures up to a certain amount on his own authority. Nonetheless, there is continual pressure from management for more conformity to its organizational ideals, even including such matters as neatness of dress, regular hours, and a more systematic approach to the coffee break. At the same time there is continual pressure from the research staff for more "freedom."

ORGANIZATION: PROJECT GROUPS OR FUNCTIONAL GROUPS?

There is a strong tendency for laboratories to become organized "functionally" into permanent specialist groups—a process whose consequences for morale and productivity are hard to assess.[5] There is some evidence that it has a depressing effect on creativity, enthusiasm, and interpersonal and intergroup cooperation.

Many forces operate so as to divide the laboratory into specialist groups. Efficient processing of common classes of problems suggests the desirability of placing together those who have expert knowledge of a given class. The principle of putting like specialists together is quite in accord with professional organizational traditions. Just as this arrangement

[4] Peter F. Drucker, Management and the Professional Employee, *Harvard Business Review,* Vol. 30 (March-April, 1952), p. 86.

[5] Paula Brown, Bureaucracy in a Government Laboratory, *Social Forces,* Vol. 32 (March, 1954), pp. 259–60.

makes for understanding by one's colleagues, it makes for objectivity in personnel evaluation by the supervisor, by enabling him to compare his men's performance on similar tasks. Similarly, it facilitates observance of such matters as the submission of technical reports and other rules and regulations. Moreover, it permits each man to be employed at the work he does best and allows him to develop as a professional specialist. It provides him with a secure status in the company, and a clearly defined path of advancement. It makes for good communication between the laboratory and other parts of the firm, through the group's permanency and the rich background of experience it obtains on the class of problems assigned to it. It makes for reliability in budgetary forecasting; with experience, the specialist group becomes able to predict its requirements.

Functional organization has its disadvantages, however. Among the shortcomings mentioned by laboratory managers are resistance to crossing specialist boundaries and cooperating with other groups; a tendency to work on ever more specialized problems of ever decreasing significance; lack of creativity and responsiveness to challenge—the scientist becomes a specialst in knowing what cannot be done and is afraid to venture into unfamiliar fields. The permanency of groups in a functional organization may be detrimental; the author and his associates found that colleague, self, and management ratings of research and development groups fell off directly with the length of time members had been associated with one another.[6]

An alternative to functional organization is organization by *ad hoc* project teams, with specialists from various fields as members, so that each team is equipped to solve a particular complex problem requiring knowledge from several disciplines. Since many problems are of this character, the project-team approach has an immediate and obvious advantage over the functional approach. The team members' energies and interests are all concentrated on a single complex problem. Such teams often turn out to be enthusiastic, creative, and dramatically successful.

Unfortunately, organization by this method presents serious administrative and personnel problems. Project teams tend to perpetuate themselves and become specialist groups, there is often strong resistance to project termination and transfer to new groups. As team members, research workers do not achieve the high degree of specialized knowledge the laboratory requires to keep abreast of advancing technology. The project tends to rely too much on its own resources, and resists consulting the literature or other specialists. Teams sometimes ignore rules and regulations and are hard to discipline. Intergroup jealousies are frequent. Too much depends upon the leadership and technical competence of the team's supervisor; men with the necessary qualities are rare. The path of promotion is not clear; supervisors have to evaluate representatives of

[6] Herbert A. Shepard *et al., Some Social Attributes of Industrial Research and Development Groups* (Progress Report, Massachusetts Institute of Technology, 1954).

several disciplines, and may lack the knowledge to do this objectively; project termination causes insecurity. Incompatibility of team members can have a disastrous effect on performance.

In most laboratories, some combination of functional organization and *ad hoc* team organization is used. Each appears to have its advantages and disadvantages from the point of view of administration, productivity, creativity, and satisfaction in work. Choosing the most appropriate combination of functional and project modes of organization is not the only organizational problem in the laboratory, however, nor does its solution automatically take care of all human relations problems.

RESEARCH MANAGEMENT: FULFILLMENT OR REGRESSION?

Studies by Donald C. Pelz and associates[7] have emphasized the importance of leadership style for the job satisfaction and effectiveness of research workers. Pelz's results indicate that dominating supervision produces apathy and resistance, and that laissez-faire or inactive leadership produces dissatisfaction and low productivity. An active, stimulating leadership role is associated with good performance and high morale. Studies by C. D. Orth and Ralph Hower[8] point in more detail to the various roles the research supervisor has to take. He must understand the needs of groups and individuals in his organization and other parts of the company. He must be interpreter of the group's needs and achievements to higher levels of management and act as interpreter of managements' requirements to his group. He must protect the group from demands and pressures which he believes would adversely affect it. He is a source of technical guidance and judgment, of inspiration and stimulation. He must coordinate the ideas and resources of his group. Studies by Shepard and associates show the successful research group leader to be a more active social being than the unsuccessful group leader. He has many friends in the laboratory, centers his social life around the laboratory. Studies by M. I. Stein[9] suggest that the successful research group leader has certain charismatic attributes: subordinates tend to identify with him and model themselves on his pattern.

All of these observations point in the same direction: the effective research group leader is a creative, dynamic, enthusiastic person who relates easily to others. Such men are said to be rare among engineers and scientists. According to Anne Roe,[10] the successful natural scientist typically avoids interpersonal intimacy. According to H. B. Moore and S. J.

[7] Pelz *et al., op. cit.*

[8] Communication from C. D. Orth, 3rd, in seminar, Massachusetts Institute of Technology, 1954.

[9] Communication from M. I. Stein, in seminar, University of Michigan, 1955.

[10] Anne Roe, *Making of a Scientist* (New York, 1953).

Levy,[11] the engineer is usually an authoritarian who attempts to handle all problems by impersonal regulations and procedures.

David Moore and Richard Renck find that scientists and engineers typically "view the organization as confused and ill-conceived; they view management as confused and ill-advised."[12] The contempt for administrative and other nontechnical occupations is reminiscent of Thorstein Veblen and the Technocrats. Engineers have a strong desire for "status and recognition of their individual accomplishments" and also "desire to be considered an integral part of an organization."[13] The engineer "is tense. Emotions which rise are ignored, suppressed and repressed unless they are so intense that they overwhelm his self-control (which is usually pretty strong). Feelings are regarded as invalid phenomena. . . . Irritability is one of the most common expressions of the tension in engineers."[14]

This mixture of feelings and attitudes implies that the lot of the research administrator is not a happy one. The professional employee resents encroachments on his autonomy. Moreover, the emphasis on individual achievement makes teamwork hazardous. Jealousies, antagonisms, and feelings of injustice are likely to arise over such matters as authorship of reports, patents, and papers. Jealousies are likely to be accompanied by disparagement of the professional competence of the others involved, with disastrous effects on collaboration. The phenomena associated with these needs and interpersonal difficulties are the basis for labeling the professional as a "prima donna."

On the other hand, some observers report that the professional works well on a team.[15] Paula Brown stresses the research workers' earnest attempts to govern their behavior rationally.[16] Shepard stresses the free exchange of task-oriented information in the determination of status and the maintenance of cooperative relations.[17] Moore and Levy note that their engineers "seek good relations, smooth interchange and workable friendships with others, and will push their feelings back into unconsciousness if these appear."[18] The major implication of these observations appears to be that the reward system should be sensitive to and accurate

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[11] H. B. Moore and S. J. Levy, Artful Contrivers: A Study of Engineers, *Personnel*, Vol. 28 (September, 1951), p. 152.

[12] David G. Moore and Richard Renck, The Professional Employee in Industry, *Journal of Business*, Vol. 28 (January, 1955), p. 62.

[13] G. H. Metz, Management and the Professional Employee, *Addresses on Industrial Relations* (University of Michigan, Bureau of Industrial Relations Bulletin No. 22, 1954).

[14] Moore and Levy, *op. cit.*, pp. 152–53.

[15] Drucker, *op. cit.*

[16] Brown, *op. cit.*

[17] H. A. Shepard, The Value System of a University Research Group, *American Sociological Review*, Vol. 19 (August, 1954), p 460.

[18] Moore and Levy, *op. cit.*, p. 152.

in response to individual task-achievement and that the control system should permit a high degree of individual and group autonomy in technical decision making.

Rewarding research workers for good performance presents a difficult administrative problem. In American industrial society, success is measured largely by the size of the organization one controls. But not all successful engineers and scientists are temperamentally suited for supervisory or administrative work, nor would they, as creative research workers, be most profitably employed in such work. Scientists and engineers are themselves somewhat ambivalent about organizational advancement. On the one hand, it may be regarded as the only generally accepted evidence of their value. On the other hand, they fear loss of professional status and competence. Even laboratory officials who have been many years away from the bench are likely to "keep their hand in" by working on some technical project. While this indulgence may be to some extent sentimental, it is also a means of preserving professional self-respect.

Many laboratory managements have attempted to solve their own dilemma and the successful scientist's dilemma by establishing a "dual hierarchy." One way up is promotion through the ranks of supervision and management. The other way up is through special privileges, autonomy, high salaries, and impressive titles (research associate, senior scientist, and so on) which can be obtained without incurring supervisory responsibilities. Unfortunately the dual hierarchy rarely succeeds in achieving its goal. Sometimes management cannot avoid the temptation to "shelve" someone whose ability to contribute has waned by giving him such a title—thus ruining the title for others. Even under the best of circumstances, the promotion to "senior scientist" is at once a symbol of achievement, and of managerial incompetence.

NONPROFESSIONAL WORKERS: CASTE OR CLASS?

In much that has been said about the scientists and engineers in the foregoing pages, a certain snobbery is implied. Frustrated in his desires for recognition, enforced to conform to regulations that he considers inappropriate for a professional, the scientist or engineer is quite likely to be defensive about his status. Moore and Renck find that "negativism and a tendency to strike out almost indiscriminately at all aspects of the work environment"[19] are associated with the low morale they find to be characteristic of scientific and engineering staffs. One aspect of the environment against which engineers and scientists rebel is the requirement that they do work which they consider "low-level" or "nonprofessional." Many regard participation in activities involving the drafting

[19] Moore and Renck, *op. cit.*, p. 60.

board "hardware," and glassware as somewhat degrading, or, as they put it, "an uneconomical use of professional time." This attitude does not endear them to the nonprofessional workers. And given this attitude, any sign of incompetence in these matters on the part of the engineer wins him the enmity of nonprofessional personnel. Such enmities appear to flourish in functionally organized laboratories, where interpersonal difficulties can quickly develop into intergroup conflicts. In project organizations, on the other hand, there often develops an intense loyalty between the professional and nonprofessional members of the team. The professional sometimes regards himself as teacher and protector, the nonprofessional, as student and apprentice.

PROFESSIONAL WORKERS: ELITE OR PROLETARIAT?

To most of us, the term "profession" connotes service or ministry rendered out of specialized knowledge, high standards of conduct (professional ethics), and a kind of democracy as the means of social control within the profession. Altruism is the keynote. In the lay mind, and to some extent in the professional, professional values appear to stand in opposition to wealth- or power-seeking. Certainly for the members of some professions—notably clergymen and teachers—the worldly rewards have been small, and throughout the professions "commercialism" is frowned upon.

While these values are in a formal sense characteristic of professional groups, there is a good deal of variety among the professions in this respect. Moreover, there is in every profession a difference between the announced values and the values that govern in practice. The popular model of the professional visualizes a client to be ministered to, as in medicine and law. The model has to be distorted almost out of recognition if it is to include the members of the engineering department of a large firm. A kind of equalitarianism among the individualistic members of the profession is also part of the popular model: the law partnership, the medical clinic, the university faculty, and the professional society being representative organizational forms. Again, this aspect of the model is abandoned in the engineering department of a firm.

Such departures from the generally accepted model of a profession reduce the meaningfulness of the word. The criteria for professional status become blurred. A host of new "professions" have arisen: public relations, purchasing, cost accounting, personnel administration, and so forth. Even the idea of management as a profession is not unpopular, though few go as far as Mary Parker Follett when she asserts that "the business man has opportunities to lead the world in an enlarged conception of the expressions 'professional honor,' 'professional integrity.' "[20]

[20] Henry C. Metcalf and L. Urwick (eds.), *Dynamic Administration: The Collected Papers of Mary Parker Follett* (New York, 1942), p. 143.

In fact, the professional model is at such variance with the state of affairs in industry that a discussion of professionalism would seem irrelevant here if it were not for the fact that a large proportion of scientists and engineers in industry think of themselves as "professionals." As has already been indicated, this self-definition has a number of important consequences through its effect on attitudes, expectations, and reactions to the demands of the industrial environment.

Concern about the implications of professionalism has been especially keen in relation to the issue of unionism. Professional associations in science and engineering have, with few exceptions, opposed organization of their members for collective bargaining. Accordingly, growth of unions in these professions has been slow. Many industrial scientists and engineers feel that unionization constitutes a rejection of professional ideals and responsibilities and a downgrading in their status to the ranks of labor.

But powerful economic and social forces create a pressure toward unionization. The individual engineer or scientist is no longer an influential member of his company, with a monopoly on specialized knowledge needed by the firm. He is one of a hundred or a thousand such specialists. The individual engineer or scientist is peripheral and replaceable. The parallel with craft unionism is striking. The highly skilled craftsman was once a proud individual, his own master or an influential member of his organization. Just as the guild was a satisfactory association for craftsmen, so the professional society has met the needs of the modern scientist and engineer. Just as the guilds were the predecessors of the unions, though not often their parents, so the professional society may eventually give way to, or at least be supplemented by, the professional union. The means used by professional associations in the past for protecting the economic welfare of their members—primarily control over entry to the profession—may not be adequate to meet the challenge of mass industrial employment.

CONCLUSION

The reader can infer, and any laboratory manager knows, that imposing order on the never-never land of ambiguous relations between science and industry is a relatively easy matter if the author undertakes no more than a classification of areas of conflict and uncertainty. The really important and challenging job is to determine what can be done to improve the situation. To date, however, most of the formal studies of research organization have necessarily been of the kinds that describe how things are and draw inferences about how they came to be that way. Statistical analysis of data resulting from studies of current practice, or even from more selective observation of things as they are, is often an inefficient path of knowledge. Kurt Lewin once remarked that the theory of gravity was

neither conceived nor tested by such methods—and added that the best way to learn how anything works is to try to change it.

There is a crying need for a more action-oriented approach to the study of research organization. Organizational experiments as carefully conceived and carried out as the physical experiments that are conducted in the laboratory are the obvious answer—painfully obvious, surely, to persons who make a career of theory and experiment. Every laboratory in the country unwittingly produces quantities of information bearing on organizational problems—and pours it all down the drain, through a lack of interest and skill in the design, conduct, and analysis of social experiments. There is no shortage of promising hypotheses. But to date there has been a shortage of research laboratories with a will to experiment. Perhaps the same forces that result in a limitation of the laboratory's innovative potential by defining its contribution as "technical" inhibit systematic study of the impact of personal, social, and organizational factors on the conduct of research itself.

Chapter 27

THE ROLE OF THE RESEARCH MANAGER*

Lowell W. Steele†

In a way, it is a left-handed compliment to say that research managers have an immensely difficult job, one that is less similar to other types of management work than is commonly realized. As managers they sit directly astride the problem of how to achieve a workable accommodation between scientists and businessmen. This accommodation is of central importance to the success of an industrial laboratory, yet it is not likely to occur naturally and easily. All too frequently, this role is not understood by scientists, by businessmen, nor unfortunately by the managers themselves. Moreover, they bear a very personal psychological burden: an ambivalence between an earlier career in technical work and a present career in management. This ambivalence frequently creates personal problems and interferes with the effective accomplishment of work.

The ambivalence arises because of the distinction, and it is a real one, that is made between research and the management of research. The separation, one might almost say the alienation, of research and the management of research is more striking than that in any other function of business. If one examines the American Management Association list of publications for any of its divisions, the work performed in a particular function seems inextricably entwined with the management of the function. Yet literature on research management does not contain many references to Maxwell's equations, or dislocation theory, or the chemical bond. The management of research is a thing apart from the work of research.

Some may object that a solid grounding in science or engineering and personal experience in research and development work are absolute prerequisites for any successful research manager. True, but at some point in the management structure the emphasis must shift to matters involving the basic purpose of the laboratory as a part of the supporting organization, the availability of necessary resources to operate the laboratory, the relation of the laboratory to the supporting organization, and the utiliza-

* Reprinted by permission from *General Electric Research Laboratory Bulletin*, Spring, 1962.

† General Electric Research Laboratory, Schenectady, N.Y.

tion of the laboratory's outputs. This work can be performed by one man or many, but when undertaken it is a thing apart from the research itself.

Some managers deny the need for this role and insist that their main responsibility is to guide and direct the technical work and provide personal inspiration to their people. Such a role is possible in the long run only if somebody else assumes the responsibility for the other management functions. The matching of individual and organizational needs and the establishment of the relationship between the laboratory and the rest of the company simply is not automatic.

Furthermore, even when such an approach is used, certain long-run problems must be considered, not the least of which is the provision of "second generation" competence. Great scientific leaders often fail to produce their own successors. Indeed, competent, highly motivated young scientists may chafe at the burden of competing with well-established men and go elsewhere.

A more insidious danger arises from the fact that such a man is trying to keep a foot in two camps without really declaring himself. He may continue actively in research, but he does have some business functions to perform or at least review. He also comes into more or less steady contact with businessmen, with their separate mores, values, self-images, etc. He is exposed to, and in fact compensated in terms of, the reward system used in industry. In a word, he will tend to accommodate, but he probably will do so gradually, subtly and unconsciously. A young mathematician recently said, "Research management carries within itself the seeds of its own destruction. If a man stays in management, sooner or later he will reach a point where he no longer understands research or the people who do it. It is inevitable. It would happen to me, too." I believe there is a very painful truth in this observation.

If the research manager consciously and explicitly switches careers, he faces still other serious problems. I know of no other function in industry in which the decision to become a manager looms so large in a person's career plans. In fact, in most functions it probably would hardly occur to many managers that they had ever made a choice. Rising within the managerial structure of a function such as finance is not viewed as deserting one's profession; rather it is considered evidence of success. I've never heard a manager of finance say wistfully," I've often wondered if I'd have made a greater contribution by sticking to the accounts receivable ledgers." The promotion of such a man is not viewed as a delicate balance between his contribution on the old job and the new one in management. The major question is whether he is competent to take on the new assignment.

Scientists, on the other hand, nearly always view the decision to enter management as a major one, difficult to make and having significant consequences on their entire lives. They have spent many years in intense

preparation for a given career. Their friends, their colleagues, their interests and tastes all revolve around science and the carrying out of research. Entering management means desertion, in a sense, and it will be so viewed by their former colleagues. Furthermore, even though the decision is made after careful consideration, certain vital consequences are rarely considered.

Professor Leon Festinger, of Stanford, has done considerable work in recent years on the formulation of the "theory of cognitive dissonance." Certain aspects are directly relevant to this problem. Very briefly, the pertinent parts of the theory postulate that the requirement to choose between two desirable alternatives creates discomfort or dissonance and sets in motion drives to reduce this dissonance. Various alternatives are available for accomplishing this reduction, of which two are important for us: (1) looking for areas of greater overlap between the alternatives so that they seem less like real alternatives; (2) looking for information that reduces the significance or value of the alternative not chosen so that it will seem even less desirable.

Reducing the Dissonance

A scientist deciding to enter management is faced with precisely this type of problem. Since he has made a heavy investment in a career in science, if he gives it up for something different he must justify his choice. Unfortunately, almost any action he takes may reduce his effectiveness as a manager unless he sees his role clearly, recognizes the emotional consequences of his decision, and takes steps to overcome them. Some managers try to make the choice seem like no choice at all by finding more and more similarity between businessmen and scientists and between research and other types of activity in industry. There are, of course, similarities and it is important to find them. But the effect of doing so is likely to be largely ineffective if the effort arises primarily from a need to solve one's own personal problems.

Other managers seek to reduce their dissonance by depreciating the attractiveness of a life in science. They frequently turn very practical, they become very company oriented, they emphasize the scientist's need for facilities and for having his outputs utilized by the company, and they also just plain reduce their contact with scientists to reduce the amount of information they receive and thus avoid reopening old wounds. A physicist recently told me about a certain manager who, he is convinced, avoids attending professional meetings and visiting university laboratories out of a sense of guilt because he is no longer a contributing member of the profession. Again, many of these areas of attitude and behavior are appropriate ones in which to make adjustment, but the adjustment is likely to lead to ineffectiveness if it arises out of a need to solve personal problems.

Thus we find a paradox: the decision to enter management sets in motion forces which will tend to impair one's effectiveness as a manager. Much of a research manager's success arises from the sensitivity and perception he exercises in helping scientists to find common cause with the company. Unperceived shifts in his own point of view will inevitably impair his effectiveness. Just as inevitably, failure to recognize the nature of his psychological role in achieving an accommodation between scientists and businessmen will create personal problems which inhibit his impact as a manager. A realistic perception of one's psychological role is of immense value. Many of the frustrations and tensions research managers feel arise from a failure to understand this feature of their function. Furthermore, if they understand it they are better prepared to cope with the personal problems generated by their role.

The successful manager must recognize and accept the fact that he is constantly vulnerable to losing his sensitivity in understanding research men. He cannot eliminate this danger introspectively. Something must tell him he is making this shift; he must have feedback from his environment. Here he is critically dependent on those in his group. A successful manager *needs* tough-minded, critical, outspoken, high-status people in his group who can help him to keep his perspective on himself. A wise old church official once said that his greatest difficulty in dealing with the ministers under his jurisdiction was to keep them from beginning to think they were God Himself, because they had nobody around to cut them down to size occasionally. Highly respected people who are not competing for jobs in the management hierarchy can aid the receptive manager immensely by pulling him up short in his endless search for accommodation. Incidentally, by encouraging the development of people with this competence and stature, and by using them in this way, he will be enlarging the opportunities for satisfying their need for esteem and influence and thus expanding the attractiveness of a career in research. The role of the high-status scientist in industry needs much greater clarification and this feature of the role is an important one.

Professionally Homeless?

This discussion has emphasized the value of a clarification of the research manager's role. This clarification encompasses far more than any given individual and the context in which he operates. Research managers in a very real sense are professionally homeless. They are all too frequently regarded as outcasts by their former colleagues, as deserters from science and technical work. The value of their contributions is thus depreciated. That research managers are defensive about this is reflected in their frequent comments that they are "overhead," "part of nonproductive labor," etc. On the other hand, because of their identification with research, a function that is relatively detached from the daily pressures

and problems with which other types of managers attempt to cope, research managers are frequently regarded as still essentially scientists by possible colleagues in management. They are sometimes viewed as "amateurs."

Whom do they have to talk to? What goals and aspirations do they have and how can their needs for recognition and esteem be satisfied under these circumstances? It is not now clear exactly what the rewards for becoming a research and development manager are. The opportunity to clarify a role, which is interpreted on the one hand as a scientist with a few extracurricular managerial duties and on the other as just a "paper shuffler," is extraordinarily challenging. Not until this is done can that role be more widely accepted by both scientists and businessmen as a demanding one, requiring the skill and competence that warrant respect.

Chapter 28

WORK ASSIGNMENT AND INTERPERSONAL RELATIONS IN A RESEARCH ORGANIZATION*

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RESEARCH AND DEVELOPMENT has burgeoned into an industry in its own right since World War II, with an annual investment in the mid-'60's in excess of 15 billion dollars. That is close to 3 percent of the Gross National Product.

It is of particular interest from the sociological standpoint that this new industry reverses the usual order of priorities and places research ahead of production. In such fields as military electronics, atomic energy, and space exploration the skills and interests of research men outweigh, in relevance for high policy, those of production specialists. Scientists are not relegated to minor staff roles; they have an active and responsible voice in the councils of top management. Not infrequently they *become* top management.

This points to an organizational revolution in a leading branch of industry. Among the many problems for understanding presented by this development, I have focused on one: the changing social character of the scientist.

The typical scientist of the past was an academic professional man, working at his own pace and on his own problems in a small university laboratory. Now he is more likely to be the salaried employee of a big industrial research organization.[1] Most of his former leeway in deciding

* Reprinted by permission from the *Administrative Science Quarterly*, March, 1963, pp. 502–20.

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[1] This is a contradiction, as Veblen would have it. He states categorically that "no scholar or scientist can become an employee in respect of his scholarly or scientific work" (Thorstein Veblen, *The Higher Learning in America* [New York, 1957], p. 63). However, there are people employed on the professional level who are called "scientists" and who do what is called "scientific work." In research and development they are typically engineers by training, but to an increasing extent related professional specialists (physicists, mathematicians, computer programmers, behavioral scientists, and even philosophers) have found a place on the "team." In

what needs researching and how it will be done is gone. Oriented as he is by an ideal of conduct suited to the "handicraft" mode of research practice of the past, he faces the necessity of adjusting himself and his professional self-image to the realities of the new, bureaucratic phase of "factory production." The bargain struck is this: in return for larger opportunities to practice his craft, access to the facilities he needs, the scientist must accept becoming an "organization man."[2]

The larger the organization grows the more it stamps the situations in which work-related human interaction occurs with its spirit and structure. From the individual's standpoint work and interpersonal relations take on new, organizationally defined meanings; moreover, these new meanings are themselves interrelated. The purpose of this paper is to clarify the connection between the two.[3]

THE ANALYTICAL MODEL

The analysis presented below is oriented by the following propositions:

1. Advancement in the organization is contingent upon

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addition, in recent years there is a rapidly expanding use of such interdisciplinary workers as systems analysts, human factors scientists, and the like. An important factor in this growth has been the practice by military agencies of requiring contractors to maintain an adequate capability in this area.

[2] Initially an important reason for putting as much administrative distance as possible between research and development work and production was to escape the supposedly stifling effects of bureaucracy on scientific creativity. For example, Project RAND (later to become the RAND Corporation) was inaugurated in 1946 with the aim of keeping together at least some of the team of scientists assembled during the war to work on research and development problems for the Air Force. It was feared that with their traditional distaste for bureaucratic and business-oriented industrial laboratories, the scientists would feel that their patriotic duty had been done and would take the first opportunity to return to the universities. The RAND Corporation hoped to provide a work environment that was free of bureaucratic regimentation and narrow, business-minded practicality, The essential idea was that RAND should compete with the universities by fostering its own "academic atmosphere." For a brief journalistic account of the growth of research and development organizations of the RAND type, see Edward L. Katzenbach, Jr., "Ideas: A New Defense Industry," *The Reporter*, Vol. 24 (March 2, 1961), pp. 17–22.

[3] Representative studies that have a bearing on this question include: Louis B. Barnes, *Organizational Systems and Engineering Groups* (Cambridge, Mass., 1961); Simon Marcson, *The American Scientist in Industry* (New York, 1960); Robert T. Livingston and Simon H. Milberg (eds.), *Human Relations in Industrial Research Management* (New York, 1957); Donald G. Moore and Richard Renck, "The Professional Employee in Industry," *Journal of Business*, Vol. 28 (January, 1955), pp. 58–66; Donald C. Pelz, "Some Social Factors Related to Performance in a Research Organization," *Administrative Science Quarterly*, Vol. 2 (1956), pp. 313–20; Herbert A. Shepard, "Patterns of Organization for Applied Research and Development," *Journal of Business*, Vol. 24 (January, 1956), pp. 52–58; Paula Brown, "Bureaucracy in a Government Laboratory," *Social Forces*, Vol. 32 (1954), pp. 259–68; Paula Brown and Clovis Shepherd, "Status, Prestige, and Esteem in a Research Organization," *Administrative Science Quarterly*, Vol. 2 (1956), pp. 340–60. For relevant literature published prior to 1953, see Bernard Barber, *Science and the Social Order* (London, 1953).

   a) demonstrated competence (a technical factor), and
   b) personal suitability (a social factor).
2. But the opportunity to develop and demonstrate technical competence and personal suitability is contingent upon having a "good" work assignment.
3. In turn, getting a "good" assignment is contingent upon
   a) demonstrated competence, and
   b) personal suitability.

The key terms in this scheme are "opportunity," "competence," and "suitability." Opportunities are scarce goods and subject to monopolization. Competence and suitability (although on different grounds) are matters of judgment—there is always the question of relevance in any given instance. All the organization formally requires is that the vacant position be filled by assigning to it a man whose qualifications meet minimal formal standards. This makes it quasi-legitimate (on grounds of prerogative) for the work assigner to act idiosyncratically without fear of close review in selecting an assignee. In the typical case there is an effective scarcity of jobs ("good" work assignments), an effective abundance of candidates, and a will on the part of the work assigner to favor those he knows and likes or who are recommended by persons he knows and likes. The result of this informal organization and control of the opportunity structure is to encourage what Goffman has called "appearance management." The ambitious and resourceful individual finds it expedient to approach others ostensibly in order to get functionally necessary information and advice, but actually to make contacts and establish relationships which may be helpful when it comes time to maneuver himself into a position to get a choice work assignment. He is limited, however, in his ability to rationalize maximally his interpersonal relations for this purpose by the functionally prescribed relations growing out of his current work. An additional hindrance is the ethical sense of how one properly relates to work and to colleagues held up by the body of professionals of which he is formally a member.

In brief, work assignment influences interpersonal relations, and yet at the same time interpersonal relations influence the assignment of work. The meaning of a "good" work assignment is that it is both the symbol of an accomplished advance and the precondition for further advance. Any work assignment, by selectively structuring the individual's work relationships, generates both constraints and opportunities. Even though the constraining features are reinforced by the scientist's need for a tolerably easy professional conscience and the good repute of his colleagues, he is strongly motivated to take advantage of the chances for appearance management available to him. Whether or not he is judged "technically competent" and "professionally suitable" for a good work assignment depends in part on his cultivating helpful relationships.

The body of this study consists of a typology of work assignments together with an analysis of the corresponding interpersonal relations.

This is preceded by a description of the environment of research and development work and the individual's stake in maintaining close and reciprocally helpful relations with others.

WORK ENVIRONMENT AND INTERPERSONAL RELATIONS

Research and development organization is peculiar in two respects.[4] In the first place, the ties that bind the research worker to the organization through intermediate echelons, first- and second-line supervision, are notably weak. Whatever organizational position he has is no more than a temporary one—he is an itinerant within the organization.[5] In the second place, the ties that bind him are remarkably strong. His relations with particular individuals are superficial and typically do not carry much beyond the immediate situation, just as his placement within any given group is tentative. "Here today, gone tomorrow," for example, an ethos self-defensively flavored with gallows humor, is the realistic and appropriate one.[6] Even so he feels bound by an ethical code, implicit but none the less potent, which compels him to give all possible aid to a colleague. *His self-respect depends on feeling ethically obligated on grounds other than that of functional rationality.*

These two characteristic features of the work environment, organizational rootlessness and a strongly held ethic of mutual aid, have an important bearing on the question of how work assignment and interpersonal relations are connected.

Organizational Rootlessness

There is some reshuffling of personnel from one unit to another in all organizations, to meet contingencies, to build up cadres of units slated to expansion, to adjust for personality clashes, or just to meet personal wishes. Even so, transfers and reassignments are rare enough to be considered unusual. In research and development, on the other hand, reassignment is so common it is treated as routine. There are many occasions for reassignment: the individual's project is cancelled, postponed, or cut back; his unit is abolished, taken off the project, or reduced in

[4] The author was employed for several years as a human factors scientist in a research and development organization. Factual references, except when otherwise noted, are based on observations made during that period. An earlier version of this paper was presented at the annual meetings of the Pacific Sociological Society, April, 1961.

[5] Hughes usefully distinguishes between home-guard and itinerant career patterns. See Everett C. Hughes, *Men and Their Work* (Glencoe, Ill., 1958), p. 108.

[6] Here, as elsewhere, the engineers show no signs of developing a sense that they are in a position "to make the next move" (Thorstein Veblen, "The Captains of Finance and the Engineers," in Wesley C. Mitchell (ed.), *What Veblen Taught: Selected Writings of Thorstein Veblen* [New York, 1947], p. 440).

force; or he is transferred to another unit. Similarly, he may be reassigned if his unit head is transferred and the new head dislikes him or his professional specialty, or if his unit is transferred to a different department or division. There may be no place in the new set up for a man with his combination of skills and experience. That reassignments are frequent and apparently necessary (because of rapid growth, for example) serves to minimize the shock to the individual affected when one occurs, but it does nothing to lessen the feeling of rootlessness.

There is another sense in which the research worker is organizationally rootless. Aside from the tenuousness of his direct attachment to the organization (growing out of the shifting and temporary character of his work assignment), the nature of the work itself is such that he is hard to locate organizationally. He is not an official and yet his rank and salary approximate that of the lower administrative echelons; he is not a production worker and yet the organization's earnings depend directly on his output, and his value is judged by it;[7] finally, he is not a staff man in the ordinary sense and yet he is a professional specialist.

This ambiguity of his formal place in the organization has its corollary in the substantive ambiguity in scope and meaning of the task he is currently assigned. In theory he is hired to do a job requiring the straightforward application of his professional skill. In practice he finds that the job is something he not infrequently has to define for himself. This double ambiguity, of organizational place and function, is the second major factor contributing to the sense of being in the organization but not of it.

The Ethic of Mutual Aid

The scientist is a professional man, and in common with other professionals he recognizes an ethical obligation to extend courtesy to fellow professionals when requested. The general meaning of professional courtesy is that one colleague has the right to ask another for information and counsel when a need arises in the course of his professional work. The specific meaning, in the case of the scientist, is that the advancement of knowledge requires full and complete exchange of information among scientists. Both as a professional man and as a scientist, the research worker is ethically bound to respond courteously and helpfully to a request for assistance. However, the research worker is an employee and a career man as well as a professional scientist. He is much less free than the classical free professional to choose his work, his clients, or his colleagues. This difference in situation produces a significant difference between his needs and interests and those of the free professional.

[7] For a helpful discussion of whether professional workers are best considered as an elite or a proletariat, see Herbert A. Shepard, "Nine Dilemmas in Industrial Research," *Administrative Science Quarterly*, Vol. 2 (1956), pp. 307–08.

Professional courtesy as an ethical norm applies to both situations (since both involve interaction between professionals), but there is a difference in content. The kind of help the research worker needs from a colleague is broader. Calling the extended version of professional courtesy "the ethic of mutual aid" will convey its distinctive feature.[8]

Organizational rootlessness produces a common sense of plight, which, in turn, produces a will to seek and offer assistance which extends beyond that directly related to task performance. The research worker's ethical orientation as a professional serves to justify his pragmatic bent toward establishing helpful relationships by elevating the practice to a matter of principle. It is sanctioned as "the professional way." Realistically considered, however, it is also the organization way. This is clear when the content of mutual aid is examined.

The first thing the research worker on a new assignment has to do is to *become knowledgeable*. Although hired initially and assigned on the basis of his specialized skills, he soon learns that professional training alone is not enough. He must also acquire detailed, factual knowledge related to the subject of his current work. The quickest and easiest way to become knowledgeable is to find a knowledgeable colleague who already is and learn from him. This is also the safest way, since knowledge in use is already evaluated for relevance and accuracy. The alternative is to search through masses of documents, many of which are irrelevant or outdated, hard to get at, and awkward to handle because of security classification and need-to-know restrictions. After the universal experience of having to discard work because of an inaccurate assumption of fact, the new man learns to check next time with a more experienced colleague before carrying his task very far.

In the second place the new assignee learns that he must *keep current*. Nothing stands still: technology changes, requirements change, the organization of effort changes. Decisions are taken day by day which affect his project. It is essential to keep up with developments and, again, the most effective way to do this is to keep in touch with other workers. In order to make his own plans he *must* know the plans and intentions of others as they develop.[9]

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[8] The code governing interpersonal relations among colleagues has a controlling as well as a helping aspect. Solidarity generates duties which are enforced as well as rights which are defended (Simon Marcson, "Organization and Authority in the Industrial Research Laboratory" [unpublished paper, 1960]). However, my primary interest here is limited to the analysis of interpersonal relations from the standpoint of the willing-striving-seeking actor, above all the actor *moving* through time and space, rather than of the structure of norms which operates in a general way to constrain and orient his action. For example, Hughes (*op. cit.*, p. 63) speaks of career considered as a "moving perspective"; that is the feature I want to emphasize.

[9] David Bendel Hertz and Albert H. Rubenstein, "The Role of Communications in Research," in Livingston and Milberg (eds.), *op. cit.*, pp. 197–207. The kinds of information exchanged are reported by Hertz and Rubenstein to be conceptual, procedural, stimulatory, policy, and directive. That there is a relationship between productivity and interaction among colleagues is confirmed by Pelz (*op. cit.*).

The technical need to become knowledgeable and keep current provides the initial motive and the immediate rationale for establishing mutually helpful relationships with others. But the sheer technical need for information is only one of several motives involved. The research worker wants to do a creditable job, but he also wants to allay the sense of insecurity which derives from organizational rootlessness, and to place himself in position to get a suitable work assignment when his present assignment is finished. This is evident from the fact that much of the interaction that occurs during an ostensibly work-related, technically oriented conference is clearly neither. It is just "harmless socializing," satisfying in itself. But some interaction (referred to as "nosing around," "making points," "taking a sounding," etc.) cannot be understood except as a calculated means to an end.

Ethical codes legitimize conduct which is otherwise questionable. The existence of such a code, implicit or explicit, is evidence that the management of interpersonal relations is troublesome. The research and development work environment generates situational imperatives which force the individual, if he is to be rational and effective, to engage in what amounts to "unprofessional conduct." Under the guise of "technical necessity" he seeks help from colleagues which is in no way justifiable as contributing to task performance or to the advancement of science. It is plainly self-advancement.

The ethic of mutual aid is an extension of the norm of professional courtesy with an organizational twist. By redefining and extending the content of the "help" that one colleague can legitimately ask of another, the ethic of mutual aid introduces order into the competitive struggle for work assignments and makes it possible for the scientist to be an organization man without doing intolerable damage to his professional poise and self-esteem.[10]

In summary, the research and development work environment is one which is comparatively free of the structural rigidities usually found in large-scale organization. This is partly a matter of design; the assumption is that such a scheme is the one most conductive to scientific creativity.[11]

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[10] "Part of the working code of a position is discretion" (Hughes, *op. cit.*, p. 108). The surprising thing in research and development is the ease with which this reserve is breached. The ethics of mutual aid frequently has the greater potency.

[11] An argument frequently encountered in the literature is that since effective research requires "creativity," and creativity thrives on uniqueness and autonomy, the preferred work environment incorporates to the greatest possible extent the "academic way of life." See e.g., Lowell W. Steele, "Rewarding the Industrial Scientist," in Livingston and Milberg (eds.), *op. cit.*, pp. 163–75. Similarly, Paula Brown (*op. cit.*, p. 264) refers to the "academic atmosphere of friendly interchange" that she observed in a naval research laboratory. But true or not as a general proposition, such a value-charged simile serves to conceal as well as to reveal. Both research workers and research managers have a stake (for different, even contradictory reasons) in characterizing the ideal work environment as "academic," and thus the ground work is laid for a myth. What seems to be emerging in fact, as Marcson (*op. cit.*, p. 7) has observed, "is the convergence of the industrial organizational and the university models into a

But the unintended consequence is that the research worker is driven by a sense of insecurity (objectively founded in the temporary duration of work assignments) to manipulate his interpersonal relations. Manipulation becomes a means to his own advancement. He seeks to justify this ethically questionable activity by interpreting it as sanctioned by the norm of professional courtesy. His freedom to be creative in this respect, however, is limited by the structure of working relationships objectively required by his present task.[12]

TYPES OF WORK ASSIGNMENT

The research worker's first interest is to have a regular assignment within the organization. To have nonregular, "special-assignment" status is in effect to be jobless. Beyond having some regular place in the organization, however, the individual wants work which is both consistent with his professional self-image and functionally important. Regular work assignments which fail to meet these two requirements are avoided as "trivial" or "dirty" or both. A nonregular assignment is also avoided (with an exception noted below), not because of the nature of the work itself but because of the nature of the assignment.

In sum, there are two discrete axes serving to distinguish work assignments in terms of their subjective meaning: suitability of the *work,* and regularity of the *assignment.*

Work: Suitable versus Nonsuitable

Suitable work is that which makes the maximum and appropriate use of the individual's special skills and experience. The *level* of skill required should—as the research worker sees it—suit his level of past achievement; and the *kind* of skill he is called on to exercise should be consistent with his professional identity. Moreover, his status in his profession and in his employing organization should be affirmed by the nature of his current work. The qualified and seasoned man expects to have his proven ability to make judgments respected and used. He feels entitled to be consulted on matters affecting his interests (those growing out of his current work

new type of organizational model." It remains to be seen whether this emergent form will be more academic than bureaucratic, or the reverse, if that is the relevant contrast to be made.

[12] Similarly, Kerr and Siegal have pointed out that very often interpersonal relations are less a function of the human relations skills of the manager than of structural and institutional factors (Clark Kerr and Abraham Siegal, "The Structuring of the Labor Force in Industrial Society: New Dimensions and New Questions," *Industrial and Labor Relations Review,* Vol. 8 [January, 1955], pp. 151–68). For a thorough discussion of the general problem of the imperatives of large-scale organization as they affect the human factor in labor-management relations, see Reinhard Bendix, "Bureaucracy: The Problem and Its Setting," *American Sociological Review,* Vol. 12 (1947), pp. 490–502, esp. pp. 500–502.

assignment) and to have a voice in the management of his project. A good work assignment makes it possible for him to exist as a personage, not merely as a man performing a task or exercising a skill. His work validates his existence as a member in good standing of the company of peers which ideally constitutes the organized research team.[13]

Suitable work is hard to characterize because, subjectively, it is a residual category. The contented worker typically feels no need to inquire into the causes of his fortunate state. The organization scientist is moved to reflect on the meaning of his work only when there is something *wrong* with it. It is for this reason that the specifics of suitable work, as the individual experiences them, are somewhat hazy, while the characteristics of unsuitable work can be set down with precision.

Dirty Work. Dirty work, often called "dog work," is considered unsuitable because it is demeaning. A man doing dirty work sees himself as a "peon," working away at a task which may be functionally important (essential to the accomplishment of the collective task—*somebody* has to do it) but is nevertheless defined as tedious and dreary. The decisive thing is that it is not *fit* work professionally; even if it does carry real satisfactions, the individual feels compelled to disclaim them. To do otherwise would be to accept a redefinition of oneself as fit for routine labor.[14] A man doing dirty work must grumble continuously if he is to make it clear to himself and to others that he is not the routine worker that he appears to be.

Trivial Work. Trivial work is unsuitable not because it is demeaning or hurtful to the professional self-image, but because it is functionally dispensable. The man doing trivial work is not so much wrongly employed as underemployed. Dirty work may at least give the worker the satisfaction that he is performing a necessary if tedious task. Trivial work is damaging to the individual's self-esteem because it reflects on the value of his contribution to the organization. If dirty work is "coolie labor," trivial work is "bit playing."[15] The work may be "clean" and

[13] This is another self-serving myth. But unlike the myth that research workers are academicians, this one divides rather than unites workers and managers. For an analysis of a number of sources of conflict between business and professional norms as they affect the scientist, see Conrad M. Arensberg, "Research Relationships and Cultural Differences," in Livingston and Milberg (eds.), *op. cit.*, pp. 287–95.

[14] To be fit is also to be unfit, as Veblen, Dewey, and others have pointed out in different contexts. Cf. Robert K. Merton, *Social Theory and Social Structure* (Glencoe, Ill., 1949), pp. 154–55. A computer programmer upgraded to systems analyst may find that his ability to code a set of machine instructions (a relatively menial task) rapidly and accurately has all but deserted him. Hughes (*op. cit.*, p. 157) makes the same point with regard to the engineer who, paradoxically, must count himself a failure if at forty he can still "use a slide rule or a logarithmic table, and make a true drawing."

[15] Trivial work is a sort of company featherbedding. Except in the short run, the arrangement is satisfactory to neither party, employer or employee. (Featherbedding is a union-won, union-protected job right; trivial work is an organizational stratagem to maintain a skilled cadre when contracts are slow. In the latter instance the initiative

require a certain amount of professional competence, but its importance to the accomplishment of the collective task is marginal. The mathematician solving problems on the level of high school geometry on a virtually moribund project feels not so much badly used as frustrated. He feels that his fate in the organization depends on his performance, and resents the lack of opportunity to show what he can do.

Assignment: Regular versus Nonregular

Accounting and administrative logic in the modern organization require that a distinction be drawn between employees occupying established and functionally related positions and those who do not have a job title. A regular assignment locates the individual within the organization *functionally*.[16] Whether in services or production, the duties and responsibilities with which he is charged are capable of being deduced from the mission of the organization. A nonregular assignment, by contrast, locates the individual administratively but not functionally; he has a boss, but no task which is an organic part of the common effort. His duties (if he has an assignment at all, he has duties) are understood to be improvised and temporary. What he produces may be substantively rational, with reference to the organizational mission, but it is logically excluded from being *functionally* rational. This is why such employees have "special-assignment" status: to distinguish their claim on the budget and overhead facilities from the costs incurred in carrying on routine production and service functions.

Since the objective meaning of special assignment is that the individual has been taken off the "production" line (or withheld from it, in the case of the trainee), the relevant question is why the action was taken. Two subtypes are distinguished, on the basis of whether the research worker's special status is definite or indefinite.

Special Assignment: Status Definite. It not infrequently happens that, as the result of a reorganization, a man is left without an assignment.

remains with management.) The employee worries more and more about his dispensability and lack of opportunity to advance his career, as time passes; and management worries about its ability to justify his retention at auditing time. Formally and relatively he may have "great job security" (Lowell W. Steele, "Personnel Practices in Industrial Laboratories," in Livingston and Milberg, *op. cit.*, p. 350), although general reductions in force are an ever-present possibility. But whatever formal security he has is weak in substance precisely because he knows that he may at some juncture find himself faced with an intolerable situation and have to leave. The high turnover rate among research workers is subject to a number of interpretations, but that it testifies to a widespread feeling of security is certainly not one of them. My point here is that a man on trivial work is likely to feel insecure in his job, no matter what the formal guarantees are. The research worker is a marginal man, as Herbert Shepard has pointed out in "Engineers as Marginal Men," *Journal of Engineering Education*, Vol. 47 (March, 1957), pp. 536–42.

[16] A regular assignment is sought (among other reasons) because, as Durkheim wrote, "the categorical imperative of the moral conscience is assuming the following form: *Make yourself usefully fulfill a determinate function*" (Emile Durkheim, *The Division of Labor in Society*, trans. George Simpson [Glencoe, Ill., 1949], p. 43).

If his record is clear, if he is a good producer, and there is no question about the long-run usefulness of his professional skills and experience, he is given a special assignment and told to wait. There will eventually be a place for him. If, on the other hand, he has acquired a reputation for being troublesome or unproductive, it may be decided that he is more of a liability than an asset. In such an event he is placed on termination notice. But since it is not customary to discharge a professional summarily except for cause, the effective termination date is delayed some weeks or months. In the interim he is given a special assignment, although typically his duties are nominal. It is understood that he will use the time to seek new employment.

A man with this type of special assignment is definitely slated, either for a functionally related position that is expected to open soon, or for termination. Whether he is staying or leaving, his status is known; he is either clearly "in" or clearly "out."

Special Assignment: Status Indefinite. Some research workers are put on special assignment because the organization has not decided what to do with them, for the moment. They are surplus. A man in such a position is placed on special assignment, with the understanding that it is largely his responsibility to find a place for himself. The meaning of special assignment in this case is that the assignee has a given period of time to find a place for himself in the organization. He is without an assignment through no fault of his own, a victim of economic dislocation. He can draw compensation while he looks for another job inside the organization, although he may decide to look outside as well. He knows, however, that after a certain time unless he has bound an assignment that suits him, he will be offered one which does not. If he refuses too many such offers, he may be terminated.[17]

EFFECTS OF ASSIGNMENT ON INTERPERSONAL RELATIONS

The significance of the type of work assignment a research and development scientist currently holds is that assignment strongly influences the

[17] At first glance it would appear that the predicament is inherent in the project form of organization and can be assumed to be limited to it. The functional form, by providing the individual with an organizational place, should do away with it. And since there seems to be a tendency (consistent with the theory of bureaucratization) for project to give way to function as the basis of research and development organization, the conclusion seems reasonable that time will take care of the problem of the man who finds himself without an acceptable work assignment. But there are at least two objections to this view. First, in the nature of research and development work generally there is a good deal of personnel reshuffling—sideways, upward, downward, and out, whether voluntarily or involuntarily in the formal sense. Second, functional organization is not nearly as bureaucratically neat and unambiguous as it may seem. Cf. Paula Brown, *op. cit.*, pp. 60–61. The question clearly needs further research. For a discussion of the pros and cons of project versus function as organizational principles, see Shepard, "Patterns of Organization for Applied Research and Development," *op. cit.*

character of his interpersonal relations. A man on a "good" work assignment feels free and confident in his dealings with others. He finds all doors open to him when he seeks information and advice relating to his current activities and responsibilities. Moreover, he expects others to come to him when they have important information that he needs and might not already have. Likewise, he is not only willing to give help when it is needed and requested, but feels slighted if someone needs his help and does not request it. It would be inaccurate to consider this arrangement only a convenience between individuals. On the contrary, it involves rights and duties inherent in research and development work. The scientist ordinarily has a genial, easy manner when dealing with peers but this can give way in a moment to an attitude of high dudgeon when his vital right to know is passed over—for example, when he belatedly hears of a meeting at which persons were present and matters discussed having an important bearing on his project.

Unsuitable Work

The Neophyte. Dirty or trivial work is not too unsettling to the new man; his neophyte status overrides the status of his work assignment. He may even welcome a trivial first job to provide a helpful buffer between the time he completes his training and the time he must assume responsibility fully commensurate with his professional status.[18] Any work assignment, even one which is unsuitable in the long run, provides him with a reason for approaching others and establishing useful relationships. His privileged neophyte status protects him from the damage to his self-esteem that might otherwise follow revelation of the prestigeless nature of his current work. In most cases he finds his reception unstrained, even cordial, since no one's place is so secure that he can afford to pass up a chance to make a favorable impression on a man whose fate is not yet determined.

But one can claim the privileges and immunities of the neophyte only for a limited time. A man kept on unsuitable work for an extended time eventually takes it as a sign of failure or discrimination; his confidence is undermined. He may withdraw from all but essential contacts with others, to gloomily bide his time and wait for a break, or he may rush to the opposite extreme and engage in frenzied political maneuvers. In either case he becomes a source of embarrassment. If success is self-confirming, so is failure—and the man never tried is just as much a "failure" as the man who has been tried and found wanting.

[18] There may not be a period of formal training, but commonly time and facilities are allotted for briefing and familiarization. Steele states (*op. cit.*, p. 351) that typically there is no additional training. There may be little additional *professional* training, but there seems to be a good deal of more or less formalized instruction in the details of particular work and projects in progress, especially in the larger organizations.

The Senior Man. A good deal more bitter is the lot of a man on "dirty" work or "trivial" work whose professional status is high and who has known better days in the organization. His pay is the same as before; his occupational classification is the same; for the time being, his friends are the same. Moreover, in many cases he is reassured that the new assignment is merely an expedient and is in no way related to future assignments. He may even believe that the reassurance was given in good faith —as, indeed, it *may* have been—but he still feels betrayed and humiliated. The easy air of equality and self-confidence he formerly wore with his significant peers becomes increasingly strained. His colleagues' greater access to prestigeful inside information (new contract possibilities, contemplated reorganizations, forewarnings of all sorts) and their more glamorous way of work life (luncheons with visiting dignitaries, cross-country trips to conferences, prettier and/or smarter secretaries) ultimately becomes a strain. Like the past-term neophyte, he becomes an embarrassment. His associates still seem friendly but more and more they are not available to him. In the extreme case he alienates his present work associates (whom he cannot help but despise, especially his functional superior) to the extent that as an aftermath of the next reorganization he finds himself without a regular work assignment.

Nonregular Assignment

A special assignment signifies nothing, because it is designed that way. But everyone knows, who is entitled to know, whether the man is "in" or "out," or whether his status is not determined. Knowledge that the assignee is "out" does more to lessen strain on interpersonal relations than to increase it. Aside from the awkward moment when the news is first known, everything favors enhanced cordiality. Information and advice with emphasis on outside employment prospects (although inside gossip is by no means excluded, since the recipient has assumed the status of what Goffman calls a nonperson) may be given more freely than before. This spirit affirms the solidarity of professionals and is a means of speeding the departing worker on his way without undue bitterness.[19]

Similarly, if the meaning of a special assignment shows clearly that the assignee is "in," strain may be lessened in that previous uncertanities about his prospects are resolved, at least temporarily. Furthermore, he may find that his stock has gone up by virtue of the vagueness of his promised assignment. His new status resembles that of the neophyte in that, as one whose fate is yet to be determined (but whose prospects are good), he should be treated prudently as a candidate for a key spot. He is

[19] See Erving Goffman, "On Cooling the Mark Out: Some Aspects of Adaptation to Failure," *Psychiatry*, Vol. 15 (1952), pp. 451–63. Additional theoretical material relevant to this discussion may be found in the same author's work, *The Presentation of Self in Everyday Life* (Garden City, N.Y., 1959).

in an ideal position to receive confidences and help of all kinds. After all, special assignment is highly desirable when it means that the assignee is being held in reserve for something better than the functional assignments currently available.

A special assignee whose status is known to be indefinite presents a strained front and is given a strained reception. He knows that if he stays, he could jeopardize his chances by a show of all-out urgency in his efforts to find a secure place. If on the other hand, he is tentatively slated for dismissal, he may find himself on termination notice before a regular assignment develops. As long as there is a doubt about his status his relations with others are bound to be tension ridden, because his future depends on the judgments and decisions of others. To be unsure of one's position is to be unsure of how to make an approach, even to one's old friends.

But even worse than being unsure is to be sure that, unless you can find someone willing and able to make a place for you on his staff by the twenty-fifth of the month, you had better start looking around outside the company. If an uneasy man gets a strained welcome wherever he goes, the prospect of a call by a man pleading in effect for a stay of execution is enough to cause even his old friends to make themselves hard to reach. This is especially true if the unfortunate petitioner has been marked as undesirable—"has the Indian Sign on him," in the sardonic orgman phrase—and is unaware of it, even though for everyone else the word is out.[20]

CONCLUDING REMARKS

We have seen that the new scientist learns to value his interpersonal relationships as a means to the end of getting a "good" (i.e., prestigeful) work assignment, and, conversely, to value his work assignment as a means to the end of establishing "good" (i.e., helpful) relationships.

Reevaluating things conventionally defined as good in themselves as good primarily as means to an ever-receding end ("success") is inevitably demanded by bureaucracy. It is a form of disenchantment, in Weber's sense, and thus is functionally rational. The emergent reality is that position in the organization is largely a function of work assignment, and the ability to do the work calls for skill in managing relations with people as

[20] A man's prospects are mostly determined by his placement on one of four unwritten lists: "fair-haired boys," "problem children," "nobodies," and "unknowns." The first consists of names that frequently occur to those in a position to offer desirable work assignments. The second is a black list. The names on it can be counted on to be vetoed by someone if they come up for discussion as candidates for a good place. The third consists of unambitious but inoffensive individuals who are going nowhere, know it, and do not care. The fourth is residual and placement on it is temporary. The ambitious newcomer strives for the first list but not infrequently winds up on the second. In his effort to push ahead he runs the risk of becoming involved in a personality clash. Few can survive this.

well as in manipulating symbols. The new organization gets the organization men it needs and wants by structuring its incentives so as to embody this principle.

Individual mobility within the organization occurs, in this perspective, as the result of the operation of two informal processes: filtering-out and building-up. But in between these terminii is a zone for watching, testing, and labelling. Here the individual, following his cues, takes the initiative and practices self-development and self-presentation. In due course he acquires the beginnings of a reputation. If it is a favorable one, "growth" is said to have occurred. He takes on the organizational identity of a "comer."

The individual experiences this as learning "the name of the game," how to play it, and that it is important to keep distance between oneself and the work one currently does and the people one does it with. He recognizes that the penalty for failing to preserve an attitude of detachment, for becoming too committed to a relationship or a work project, is to become typed as "strange." If he wants to move up he acts accordingly.

The old scheme of values associated with scientific research is thus irrelevant in the new setting. The social character anchored on those obsolete, pre-bureaucratic values is itself obsolete.

The immediate significance of this conclusion is that it casts doubt on the accuracy of some widely held administrative assumptions, and on the relevance of much of conventional organization theory.

The typical administrator assumes that the scientist is like a skilled craftsman or technician: assign him a job and he will do it, or hold him in reserve and he will wait. Similarly, the main, structural-functional strand of contemporary organization theory projects a model of the organization as made up of relatively fixed positions. The individual incumbent has a status (or statuses) and enacts a role (or roles). The life of the organization consists in people doing more or less what is expected of them.

What the practical administrator and the organization theorist have in common is that they see the scientist as a conscientious skill-exercisor and dutiful role-player. This traditional picture hardly fits the situation of the highly volatile research and development organization of today. The scientist exercises skills and plays roles, to be sure, but this is much less decisive as a determinant of his character and conduct than the fact that he has to manage his interpersonal relations in order to get the chance to use the skills and play the roles he wants.

The life of the organization, in truer perspective, consists in individuals rationally seeking their own advantage under conditions not of their own choosing. The individual does not stop at the point of passively adapting to those conditions, any more than he stops at reactively adjusting to demands made upon him by virtue of the position he holds. He goes much further, and actively manipulates the organizational apparatus

in pursuit of his own ends—just as he manipulates others, his relations with others, and, finally, his self. Practice and theory ought to take account of this fact.

The social character of the scientist seems to be changing. The wider significance of this conclusion derives from the revolution of priorities mentioned at the outset. As research and development becomes at least equal to and perhaps ascendant over production in its ability to attract capital and talent, more and more innovating, individualistic, inner-directed, academically oriented scientists, hoping to find larger opportunities or simply to avoid being left behind, enter research bureaucracies and become transformed into organization men. There the individual discovers that any given piece of work has an organizational meaning which overlays and obscures its intrinsic meaning, and that, if he is prudent, he must learn to act on the basis of the organizational meaning. The price he and his professional type stands to pay, therefore, for the privilege of being admitted at last to the seats of social power, is nothing less than the loss of their right to define what is scientifically problematic.

Whatever the long-run consequences, for science and the scientist, of an institutionally fostered mental set that consistently discounts the reality and value of the thing-in-itself in order to meet the necessities of organization life, one thing is clear: it does not harmonize well with the "cult of idle curiosity" that Veblen took to be the essence of the scientific spirit.[21]

[21] *The Higher Learning in America*, p. 128.

Chapter 29

CASE STUDIES OF THREE COMPANIES: BARRIERS TO INNOVATION IN THE LARGE ORGANIZATION*

MUCH OF THE discussion dealing with creativity and innovation in the business firm has tended to offer the reader a choice between believing in the omnipotence of the creative individual or considering him a passive instrument of organization forces. The choice is too narrow. In the most brontosaurian types of organizations there is mutual interaction between human participants and the institution itself. In our individualistic tradition, we naturally look for the creative individuals: the Benthams in politics, the Adam Smiths in economics, the da Vincis in imaginative and skilled expression, the Galileos in the march of human enlightenment, and the Pasteurs in the overcoming of disease.

There is certainly some validity in this emphasis. However, creativity in the industrial environment is also a function of structural design, behavioral patterns, and social pressures. The innovator in the business organization is constrained by organizational structure, tradition, and inertia. The individual is encumbered by top-heavy hierarchical structures manned by managers endowed with nebulous authority and responsibility but not possessing the power of knowledge. In fact, it may even be valid to recognize that innovation is not individual at all. With apologies to William H. Whyte, innovation may, in many respects, actually be a group activity.

Yet these criticisms of the smothering of the individual in a corporate octopus of paper forms and confusing responsibilities are too simple, too superficial. In spite of existing restrictive organization pressures, the individual is important and creative. Students of organization change have recognized the organizational pressures affecting innovation, but, as one study concluded, "at all times there was someone who believed in the project and pushed it."[1]

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* Reprinted with permission from Leonard R. Sayles, *Individualism and Big Business* (New York: McGraw-Hill Book Co., Inc., 1963). The reprint is Chapter 14 (pp. 127–52) which reports a collaborative study between Prof. Sayles and Ross A. Webber of the Graduate School of Business, Columbia University.

[1] Harriet Ronken and Paul R. Lawrence, *Administering Changes* (Boston, Mass.: Graduate School of Business Administration, Division of Research, Harvard University, 1952).

We can put the matter in another way. Some observers have challenged the assumption that initiative is a trait widely distributed throughout the population, and that the barriers which society has itself erected, especially in its large organizations, alone prevent its flowering. They indicate that a strong case may be made for the claim that most people prefer to follow; that they do not have a high degree of physical and emotional energy or the ability to take risks and deviate from established ways. The questions, though justified, only further reinforce the need to look at the influence of the organization on the individual charged with at least some responsibilities for innovation. Even our cursory research indicates that interactional energy and the ability to deviate from existing patterns are very important to innovation. But we should not ignore the roadblocks set up by the organization which put a premium on these rare attributes.

Creativity is not a "you have it or you don't" proposition. It is a continuum—a fundamental common attribute of all humanity. No organization can tap all that resides deep within the individual, and we do not want or cannot expect to make things so easy and secure that even the most timid individual will have all his views accepted. Only the incredibly naïve expect such complete and immediate responsiveness of the organization to the individual. However, whether or not a broad spectrum of humanity has innovative qualities is almost irrelevant. The important point is that the organization must not operate in a manner that poses insurmountable obstacles to all but the most creatively gifted or energetic individuals.

In the social sciences, at least, we often find it illuminating to move from the general to the concrete, from ideas to specific, empirical incidents and human behavior. We have been discussing the role of the individual in the large organization. What better proof of the pudding can we produce than situations which require, or at least are designed to call upon, individual initiative and creativity?

Three graduate students at Columbia's Graduate School of Business were employed in staff departments of large corporations whose objectives were somewhat similar. These departments were supposed to be agents of change for the corporations—commissioned to produce original, creative ideas that would solve existing manufacturing problems or help develop new products. In a sense, theirs is the problem which Wilbert Moore has just identified: the institutionalization of change in large organizations—getting people to develop relevant new ideas and getting the organization to accept them and put them into practice. Change is never easy; as we shall see, it is particularly difficult in large organizations.

Thus, we have here what in the physical sciences is called a "worstcase condition," requiring the personnel of a relatively small department to be influential over larger segments of the organization which typically have more prestige and a comfortable way of doing things.

Ross Webber, one of the three students mentioned and a PH.D.

candidate at Columbia, has added the studies of his two colleagues, Ralph Franke and Stephen Brown, to his own case materials.[2] However, as the reader will observe, Webber has been careful to preserve the point of view of each of the writers. We have here an opportunity to view the world of change and resistance to change from the point of view of the young engineering manager. Each is eager to show what he can do in problem solving and initiation of change; each is faced with the resistance of the organization. How does the world of work look to them? How do they rationalize their successes and failures, and more importantly, what are the common organizational factors in each of their experiences that can be identified?

Here we are in a unique position: watching highly ambitious, intelligent, technically trained young men trying to make their efforts felt in large compartmentalized organizations. What are the organizational roadblocks deterring creative responses to problems? Which are inevitable in the large organization? Which are desirable and which intolerable?

Ralph A. Franke has described a manufacturing organization in which he, as a plant technical engineer, was developing a complex gauge-and-control system to measure automatically the thickness of a chemical coating applied to plastic film.

Stephen G. Brown has recorded his experience as an assistant technologist in a major steel company research center. He observed relations between two groups which were engaged in similar applied research on coke and coal but which used different technologies.

The third case is a large chemical manufacturing organization. As a participating industrial engineer, Ross A. Webber has detailed the relationships between staff and line and their influence on innovative effort.

Although each of the studies was done independently and in very different types of companies, Webber, in the remainder of the chapter, shows us how each of the observers was struck by certain factors that were common to all three situations.

INTERGROUP COMPETITION BETWEEN STAFF GROUPS

Many companies make two contradictory assumptions, neither of which is very realistic. They assume that most experts—managers or professionals with technical backgrounds who share a common dedication to quantitative and precise methods of analysis—will be able to work together easily. After all, they are trained to be rational problem solvers. They also assume that these same people need a little competition to keep them working at top efficiency. These firms carry the concept of competition from the market place to the firm. They believe that competition between groups is conducive to getting the best of each group.

[2] These papers were prepared for my [Sayles'] course, "Human Behavior in Organizations."

> The company had set up a management systems development division which wanted to do work which we in industrial engineering felt was our responsibility. Plant management thought that competition between groups would bring out the best efforts of both departments.[3]

Our research among participants indicates that they looked upon competition as inhibitive and destructive. Continuing the above statement:

> But our efforts were directed less toward doing a good job than in getting assignments and keeping them from management systems development. We spent time gathering information and performing mathematical work that the other already had completed. We jealously guarded information and seldom assisted each other.

It is interesting that there was unanimity among our participants in considering intergroup competition unproductive conflict. Franke, for example, felt that his contribution in developing a new measuring gauge would have been greater if he had access to the work of certain other groups or if he were able to control the other group in the sense that his initiation would result in predictable actions by the second group.

> (Previously) I had filled a position as a development engineer in the company's engineering center and had been assigned to similar work as an officer in the Army. In both situations the work was stimulating and satisfying, and my colleagues were cordial. In both organizations the service functions like maintenance, accounting, drafting, and model shops had been at our beck and call. . . . Because of this experience I was unprepared for the troubles I encountered on my first job assignment after discharge from the service.
>
> After a great deal of pressuring and bargaining, the gauge (which I was developing) was installed. However, it was not electrically wired upon installation and was still inoperative. Over two months elapsed without the wiring being connected. I would talk to production, and they would say, "See maintenance, they are the ones that have to do the work." And maintenance would say, "Talk to production. They won't shut down the equipment so that we can do the work." My supervisor would then put the pressure on: "What's holding up that project?" I would growl at the janitor when he came in the office, but what could I say or do? If I had the authority to tell those maintenance people to connect the wiring we could make some progress!

There is a widespread temptation among staff groups to interpret intergroup relations in conspiratorial terms: "The other staff groups are out to block what we are doing because for some reason they don't like us."

> I was a member of the plant technical group who were to work closely with production on both their short- and long-range problems. We were supposed to be the problem solvers: to find out what's wrong and decide what to do about it by running necessary tests, and then write specifications for equipment changes or additions. The specifications went to the project engineering group for the

[3] The passage quoted above and others that follow are excerpts and synopses from the reports of our participant observers. No footnote references to the individual studies are given.

detailed design work. These specifications were a constant source of conflict between the two groups. PEG would send them back because of insufficient information and technical would claim project wanted us to do their work. Or PEG would complain technical didn't know what they were talking about, or hadn't run enough tests to justify the installation. Then technical would claim that PEG was trying to push the responsibility for possible failure off on them when it was all due to incompetent design on their part. Many volumes could be filled with examples of charges and countercharges. . . . In my view, most of the ill feeling was directed from the PEG to technical, not vice versa.

Brown has offered the suggestion that it is not competition per se which is disruptive but competition combined with fancied status differences.

I was employed as an assistant technologist in the coal-and-coke division of a steel company research center. The division was divided into two groups—Don Henredon's and Al Cohen's. Basically, both groups were supposed to test raw materials on various characteristics and write technical reports. However, their methods were based on different types of technology. . . . I became aware then of an intense rivalry existing between Don and Al's groups.

Technological differences in approach to very similar problems were the original reasons why two separate groups had been created within the division. Several years ago, all coal-and-coke testing and research was based on the method of empirical tests in the pilot coke ovens. Oven results were then used to predict the qualities of coal and coke from various sources. This method is still used by Don's group. Al Cohen became a member of this group and began to work along different technological lines. In summary, his approach endeavors to predict coke qualities from microscopic examination of coal and coke. Management split the new techniques off from Don's group and created a new group. At first the original group did not see any threat in this action. They didn't do anything either to boost their own standing or to cast aspersions on the new section. However, management began putting undue emphasis on the new technology. Cohen's group was taken out of the C building quarters it had shared with Don's group and was given an office in the A building. This was the headquarters of the research center, was air-conditioned, contained the cafeteria, and was the center of important staff activities. In comparison, the old coal-and-coke group had been placed in C building near the rear of the research center because pilot ovens dirtied things up. The building was only infrequently visited by the director, was not air-conditioned, and housed no staff groups. From it one often had to walk to the cafeteria in the rain. Placement in A building was recognized as a symbol of considerable status.

Don's group began to be deeply critical of management's treatment of the new technology, of the superior prestige given to the group in conferences, and so forth. Often I heard Al's group referred to in unkind terms. Technologists in our group told me that the new technology wouldn't come near the correct results if we didn't run the tests our way first and submit our results along with our sample to them. There was even considerable talk about intentionally sending them erroneous reports to see if they would make errors. We were told (only half jokingly) that the main purpose of the new group was to take pretty pictures which would be shown to visitors and explained at professional conferences to impress competitors with the esoteric work that the company was doing in the field. In the opinion of our technologists, Al's group had only limited usefulness, and they were being accorded far too much recognition by research management.

I believe that the basic reason for this continuing conflict was the great status advantage accorded the new group. The technologist in Henredon's office had convinced themselves that Cohen's approach was technologically inferior to our own. If the status differential were to disappear, it is likely that as time passed, the new techniques would be accorded equal technical validity by our technicians.

Status differences between staff groups become even more destructive when the organization-caused status differentials clash with what society normally considers proper. For example, (1) older people are generally accorded more status than younger men; (2) if one group initiates work for another group that responds with a service, people think the first group has higher status; (3) if one group continuously has relations with a group of recognized high status (such as top-management personnel) while a second group does not, the first is thought to have higher status.

We see some similar problems in Franke's comments on relations between his plant technical group and a project engineering group.

The plant technical group worked closely with production and wrote specifications for equipment changes or additions. The specifications went to the project engineering group for the detailed design work. These specifications were a constant source of conflict between the two groups. . . . The PEG engineers were all older and more experienced than the technical engineers. I think they resented these younger, less experienced engineers telling them what to do, and also resented having to follow their instructions. The technical engineers also had wide contact with all levels of supervision in the plant. They were consulted by these people on problems and regularly made presentations to them. Consequently, they were well known by the people who counted, and most of the promotions to responsible supervisory positions went to them.

In summary, groups of experts find it easy to be competitive in the large organization where there is competition for status (no special pains to create it are necessary), and one cannot easily justify the assumption that competition, per se, is good for productivity. However, one caveat! We are still viewing the world from the point of view of the participant who feels frustrated that the organization does not conform more to his needs as a creative source of ideas and change. We cannot know how much of this added tension (and sense of competition) had useful by-products in spurring individuals to greater effort. Superficially at least, it seems doubtful that these goals could compensate for the losses incurred as a result of the deliberate blocking of the efforts of one individual by another who happened to be situated in a competitive group.

But another type of intergroup conflict frustrates individual initiative. We move from relations between expert or staff groups to the day-to-day work relationships between staff and line, between the individuals who are supposed to identify and solve problems by introducing changes and those who are supposed to absorb or use these in carrying on the major business functions of the organizations.

INTERGROUP COMPETITION BETWEEN STAFF AND LINE GROUPS

The very term staff and line denote the traditional concept of the relationship between the two groups. The staff man is a box on the organization chart to one "side" of the line supervisor. He and everyone else is supposed to recognize that his efforts have no meaning aside from their use by the line manager he is serving. Yet over and over again, experience indicates that things are just not so simple. Too often the very objectives of these two types of groups seem to be in conflict.

One of our participant observers concludes wryly that what he did as a staff engineer seemed unrelated to what the production foremen were doing. The latter considered Franke's "assistance" and new equipment, which was supposed to help them, merely intrusions on their responsibilities.

> During the next couple of months I spent at least sixty hours a week in the plant. The pressure on production to get the equipment installed was now turned on me to keep it in operation. A great amount of trouble persisted with the measuring part of the system. The production foreman was unwilling to take charge of the equipment and operate it as a production piece of equipment. The usual phone call would come at ten o'clock at night and I would hear, "Your equipment is not working. Come in and take a look at it."
>
> A great percentage of the trouble was imagined or purposely blamed on the equipment to "explain" other troubles. Because of other pressures on the foremen, they constantly were looking for things on which to blame operating trouble.
>
> This was a common problem for engineers. Test equipment would be blamed for every problem under the sun, and good ideas would often go down the drain for no other reason. An engineer would try to do the best possible job of explaining or "selling" the foremen and operators before the equipment was tried. This was difficult to do however, since production involved a three-shift, seven-day operation. Furthermore, most of the engineers believed that the foremen would not understand very much of the explanation since none of them were technically trained. An engineer would often come in one morning to find that the equipment he had worked three months to design and get installed had been removed because "it gave us trouble." Usually, the trouble they experienced on the run seemed totally unrelated to the new equipment.

Underlying this observer's comments seems to be a wistful inquiry, "Why should things be this way?" "Why can't production people realize that I want to help them?" However, as with conflict between staff groups, one cannot simply explain the difficulties by blaming misunderstandings caused by poor communications and the like. There are more tangible reasons for lack of cooperation.

Staff has ceased to be related to line in a purely traditional advisory relationship because of the emergence of a kind of gap between knowledge and authority. As many students of organization have pointed out, innovation, authority, and capacity to act have become increasingly fragmented in modern complex organizations. The fast pace at which

knowledge accumulates has more and more concentrated technical ability and experience necessary for innovation in staff groups, not line groups.

> To an increasing extent, for reasons of technical specialization that are well known, innovation is in the first instance a staff function and not in any ordinary sense an executive or entrepreneurial one. This specialization, however, heightens the universal tension between information suppliers and decision makers, that is, thinkers and doers, or in administrative language, staff and line.[4]

This fragmentation has initiated a trend toward isolation and professionalization of staff groups concerned with innovation. Staff managers become concerned with the special goals of the staff organization (e.g., increased budgets) which are not necessarily related to the needs of the groups they are advising or servicing. Webber, the observer of an industrial engineering division, comments on the role of selling staff services in the company in which he worked.

> Industrial engineering, as a separate department, has formulated goals of survival for itself which are reflected in a desire to promote its own existence by obtaining additional assignments. Top staff management is constantly asking its personnel for greater effort in selling and salesmanship.
>
> Selling places emphasis on searching for problems and getting approval to pursue them. It implies virtually a complete reversal of the behavior patterns associated with being an adviser to the line. Selling requires frequent and vigorous initiation of interaction from industrial engineering to production departments. In order to have something to sell, an engineer requires a greater order of familiarity with department problems and possibilities than he would derive from passively waiting to be consulted.

However, Webber recognizes that the individual in attempting to perform his duties cannot behave completely as staff management's emphasis on selling demands.

> Geographic, social, and administrative factors emphasize the separation of IE as staff group from production. The emphasis on selling reinforces this by laying stress on the engineer as an outsider trying to get in. The engineer becomes aware that he must break down this identification of him as a stranger to production supervision. He must be accepted for two reasons. (1) Engaging in selling activity requires something to sell. Normally this something is an idea which has been developed after having a chance to observe production's problems. Cultivation of these ideas is a function of search, and the searching process requires an intimate knowledge of production activities. (2) It is usually not possible to maintain a significant level of projects only by selling. The engineer believes he must return somewhat to certain aspects of the traditional advisory relationship by cultivating requests for his services from the production department. The emphasis here becomes one of making himself expert and indispensable in certain areas so that the production department supervisor will naturally turn to the engineer when difficulties arise.

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[4] See Wilbert E. Moore's contribution to this volume [*Individualism and Big Business*], chap. 13, p. 119.

AMBIGUITY IN JOB REQUIREMENTS

William Foote Whyte has commented that one of the difficulties in communication between specialists and operating men is that because the intervention of the specialists tends to be sporadic, and unpredictable, its irregularity leads to a negative reaction from production supervision.[5] Geographic separations, social differences, and job duties contribute to this variable pattern. However, even more destructive of easy give-and-take relationships are the abrupt changes in the very character of the staff-line relationship.

An industrial engineer might act as an advisory staff to the department head when answering his question about whether labor cost was cheaper when baling scrap in plastic sheeting or putting loose parts in metal boxes. On another job the same engineer can act as staff to the division superintendent while checking on performance of department supervision in the matter of excess hours charged to a particular crew. On a third occasion, the engineer can exercise IE's authority over incentive plants by forcing him to accept a maximum labor hour limit on a new plan.

In oversimplified terms, in our case, the engineer does not know exactly where his job fits into the organization. Is he consultant to the chemical plant superintendent and production department head, or is he responsible for the installation of measured work-control plans? In fact, he is expected to be all of these. Formal company procedure emphasizes his role as a traditional advisory staff to the superintendent. Individual engineers try to gain a position of indispensability to the department head in order to encourage initiation of requests by production supervision. They hope ultimately to improve and elaborate the relationship of engineers to production to a point where it may approach a joint consultative position in the operational structure of the production department.

The growth of professionalism in the area, the development of mathematical techniques, and the desires of new engineers all complement staff management's emphasis on innovation. This leads to a search for problems and initiation from staff to line; and these contacts are not welcomed by production personnel, who believe that staff should wait to be asked in on their problem. My staff department also believes that they are supposed to control incentive plans and decide what use the production department can find for them that best fits the goals of the company (rather than what IE might do to help production supervision best meet the production department's goals). Formal policy strengthens their hand here by assigning to IE the power to approve or disapprove all incentive standards. In addition, it is in the nature of many labor-cost control plans that they require IE to act as inspector, controller, and auditor of production-department activities. Industrial engineering sets maximum hour limits for which crews can be paid and then must approve the department heads' special wage payment request—a payment necessary for any excess hours. At least one crew exceeds the hour limits almost every week. The strains this system puts on relations between the department head and IE should be obvious. This interaction is the only one that must be initiated by the department head to get IE's action, and it is just the reverse of the relationship implied in a traditional advisory theory of staff.

[5] William Foote Whyte, *Men at Work* (Homewood, Ill.: Irwin-Dorsey Press, 1961), p. 561.

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The IE supervisor upsets the relationship in another way. To him, production units are departments to be checked and inspected to see that they are contributing to company goals according to value criteria set up by the IE division. The supervisor sits in his office checking budget figures and pay-plan performance. Whenever he has a question, he notifies an engineer to call the production department concerned and find out what happened. In this event, the engineers are forced to initiate an interaction with a department head on a matter which is obviously a justification of the manager's activities. This questioning has an adverse influence on the sentiments of the department head toward the engineer making the inquiry.

We thus see that the relationship between staff and line is characterized by confusion and inconsistency in interactional pattern and organizational roles. Both the engineer and production supervisors in the example above implicitly recognize Whyte's contention that it is impossible to be an inspector and a consultant at the same time.[6] The job requirements are incompatible. Therefore the staff man is left in a state of instability and flux in his relations with the people he is supposed to serve. Innovation ceases to be the prime focus of his activity. It is replaced by attempts to create for himself a position of indispensability and regularity.

CONFLICTING GROUP GOALS

It is wrong to attribute this conflict to incompatible personalities or to explain the situation in conspiratorial terms. It is also misleading to attribute to faulty communication lack of cooperation and breakdowns of information flow. Conflict is not wholly explained by ambiguity in job definition or nebulous assignment of duties. It is frequently the result of basic incongruities in the goals of the various groups and specialists who are supposed to cooperate in producing something new.

Franke's earlier example of a plant technical group engineer attempting to install a new gauge for production-quality control indicates several examples of inconsistent goals.

> The reason for the urgency of my project was not related to any needs of production. They could operate quite satisfactorily without it, although costs would be slightly higher and quality somewhat lower.
>
> The production people recalled the number of shutdowns and delays that they had experienced as a result of the efforts earlier to develop a workable production instrument. They seemed convinced that the present gauge would never work and that it was a waste of everybody's time to play around with it.
>
> In addition, this was a difficult period for production supervision in the coating area. Since the operation was the bottleneck in the plant, the production superintendent was applying a tremendous amount of pressure there.
>
> The installation of my equipment required a shutdown of one-sixth of the area for a two- or three-day period. Supervision was not anxious to have the coating tower down while the gauge was installed.

In this case production supervision strongly resisted staff's attempts to install the new gauge. They felt no real need for this innovation. As new

[6] *Ibid.*, p. 509.

arrivals, their main concern was meeting production quotas in a bottleneck area while building up and maintaining stability in production operations and equilibrium in interpersonnel relations. The dominant pressure on them was maintenance of production at a certain rate, not cutting costs or improving quality. Certainly supervision would have liked to improve the cost picture, but for the present emphasis was on production. They considered the plant technical group and their innovation a threat to the stability they were attempting to achieve. We have already seen comments on the attitude of the foremen in refusing adequately to supervise operation of the gauge since it was "your (plant technical group's) equipment."

It was not irrational resistance to change or to the initiative of the technical specialists that prompted the production supervisors to postpone installation of the gauge system. Rather, it was a decision to deal with matters felt to be more important under existing circumstances. On the other hand, the goals of the plant technical group were not solely concentrated on realizing savings for the company. In this particular instance, Franke believes the primary motive was personal ambition.

The real push came from Phelps, the technical superintendent, who wanted to develop this system before one of the other plants. He was new to his position and of course wanted to make a good showing for the technical staff at corporate headquarters. This particular project was also of personal interest to him since it had been under his direct supervision before his promotion.

Our IE observer comments on divergent goals in similar fashion.

The goals of the foreman in solving his problems are quite different from those of the industrial engineer. The foreman is mainly interested in getting production out while maintaining stability among his personnel and equilibrium in his relations. On the other hand, the industrial engineer is busy looking for problems that are not apparent, in the hope of applying his various techniques to the modification of existing conditions. We cannot with justice say that the foremen are shortsighted while the engineers are looking ahead. The criticism is too simple, and it contains a value judgment. We can say, with more insight, that the foremen see no real need for staff industrial engineering assistance in meeting the goals defined by the job situation and rewarded by production management.

The department head is also concerned with production, but his primary area of evaluation is on costs. Certainly his point of view is wider in scope than that of the foremen, and he is interested in innovative change. However, the heaviest pressure is still on costs, and relatively short-run costs, at that. He is not rewarded so much for long-term down trends in cost as for not exceeding weekly budgets and so on. Since industrial engineering has a major part in setting these budgets, his aims in having a loose budget clash with IE's aims in showing paper savings by cutting the budget as much as possible.

The superintendent has a greater interest in long-run costs and innovation, but he is concerned with using his own staff engineers. He maintains that their employment is cheaper, that he gets better work, and that he trains his future supervisory personnel at the same time. His desire to control all innovative proposals and to promote origination of ideas within the chemical plant division conflicts with IE's expansionist aims in the methods field.

I have pointed out the desires of both IE management and the newer engineers to broaden application of sophisticated techniques and increase the power of the staff division to initiate innovation for production supervision. We

have observed that this concurrence of aims has resulted in an emphasis on selling industrial engineering services. Frequently the engineer, armed with his arsenal of techniques, will simply try to find a problem to which he can apply his expertise. In our case too much emphasis is being put on the techniques instead of on the problems.[7] This technical preoccupation is also a manifestation of too much concern with "professionalization"—increasing the distinctiveness and the sense of exotic intellectual activity associated with your field of specialization.

Staff management aggravates this tendency by concentrating interest on the needs of the IE division as a professional discipline. There is still much verbal declaration that industrial engineering is a service organization, but the emphasis has been subtly shifted away from service to the production department with its often mundane problems. Focus is on developing a discipline and serving the company standards as formulated by industrial engineering.

Number of People and Quantity of Work

As with so many other considerations, the problem of oversupply of people is related to divergent goals. The growth of bureaucracy and empire building has been well presented by others and won't be repeated here. However, Brown thinks that the excess number of technologists in the research center is harmful to the quality and quantity of effort expended. In his discussion of apathy he points out the problem of differing goals and extra personnel.

The research center saw the company goals in the light of its own goals, namely, an expansion of its personnel even when it had no place to put the new people and no real jobs to give them. As long as the top management of the company continued to make funds available for expansion, research continued to expand.

This overstaffing of the center provided soil for the growth of other problems. Communications were hampered by an excessive amount of hierarchical structure. In the center alone there were five to seven levels of management, depending on whether one was a technologist or a laborer.

Our office demonstrated the physically crowded conditions at lower levels. Eight of us worked in a common area which would have been more suitable for four. A disturbance for one became a disturbance for all. The average conversational level was fairly high, and the crowded conditions put a demand on each individual's tolerance for superfluous interaction at the expense of relations needed for work. Had we been required to increase our output substantially under these conditions, we probably would have been at each others' throats constantly.

An excessive number of people results in decreased opportunity for promotion, confusion over relationships, and distorted communications; and it often encourages the less ambitious to stay on at the expense of the more ambitious who leave.

[7] The Nobel physicist, Hans Bethe of Cornell University, has commented that the use of computers has led to a tendency to think only about how to solve problems instead of analyzing the elements of the problems themselves.

ACCESS TO INFORMATION: PHYSICAL PROBLEMS OF LOCATION

Many observers have commented on the fact that innovation requires submergence in the problem—prolonged familiarity with the elements, hard work, and extended thinking about possible alternatives.[8]

Thomas Edison invented more by analogy and experimentation than by imagination. He created by direct frontal assault—marshaling the widest array of facts and ideas and then carefully searching for heretofore unrecognized relationships among them. Yet today we realize that the free flow of information vital to this process is often hindered. In the steel company research center mentioned earlier, we see that intergroup rivalry between two staff groups performing very similar functions adversely affected the flow of information. It appears that each of these two research sections performing tests on the same types of raw material for the same company made every effort to minimize information given to the other.

> In many meetings between members of the two groups, technological aspects and comparability of the two different research methods were carefully avoided as subjects of conversation. . . . When work was given to one group by the other it was never given priority except under extreme and continued pressure. . . . The joking about sending the new group results from the empirical tests has already been mentioned.

Our industrial engineering observer explicitly speaks of information flow as one of the vital necessities in staff relationships if they are to encourage innovation.

> The major elements in the relationship between IE and the recovery department is the extreme difficulty encountered by engineers in obtaining information. I am not repeating the old "better communications" shibboleth because it is not simply a matter of two groups misunderstanding each other. The types of information desired by IE can be divided into four categories: (1) specific production data, (2) managerial directions and letters concerning methods and procedures, (3) general running production and control data, and (4) projects reports from the development engineers. This information is wanted for two main reasons. First, the engineers cannot search for and solve problems without free access to information. This means access to data and letters that they cannot specifically ask for since they don't know beforehand what is valuable and pertinent. Second, IEs want to know what is going on in order to strengthen their position as an operating member of the production department. By making themselves indispensable they hope to increase the interaction initiated to them by the department head. The engineer in pursuing this goal will profit by having as intimate a knowledge of department activities as possible, and geographic and social isolation exercises a detrimental influence on the acquisition of this information by affecting the nature of interactions and injecting physical handicaps. All IEs are located in one office situated about three miles

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[8] Cf. Selections in Harold Anderson (ed.), *Creativity and Its Cultivation* (New York: Harper & Row, Publishers, Inc., 1959).

from most of the chemical plant operations. The trips require a wait and a bus ride taking about twenty minutes. Engineers do not go out just to say hello, or to have a cup of coffee. Thus almost all interaction initiated by IEs are job-related. Indeed, engineers are generally thought of as being sticklers for work, both because of the "efficiency expert" connotation of their position and because of the job-oriented nature of their interactions.

Of course, some engineers are characterized by higher interactional energy levels than others, and these people do attempt to initiate social relations. Certain social pressures, however, hinder even these men. Age, social, and educational background differences between department heads and industrial engineers tend to be detrimental to any basis for social intercourse.

Often the engineer desires access to production records for no specific reason except that he wants to scan them or search for problem areas. Both geographic and social factors discourage the IE in this project by hindering physical access to the files, by not providing working space in the department office, and by making friendly relations with the clerk difficult to achieve. In spite of the department head's apparent willingness, the clerk has resisted giving free entrance to the files.

All these difficulties in obtaining information are not insurmountable obstacles in the path of the technical expert who wants to be a creative problem solver. In the above example, they might be overcome by frequent trips to the department, a willingness to work in inadequate surroundings in making copies of the data, and the exercise of patience. But all these activities absorb energy and diminish the engineer's attention to innovation and problem solving. It is clear that the flow of information in an innovative atmosphere consists essentially in free movement of data and memoranda, some of which is apparently superficially irrelevant, without deliberate request and without a specific purpose in mind. Valuable reflection depends on a continuous receipt of information which acts as a stimulus to the encouragement and direction of thinking. By blocking information, even a minor official can hamper the innovative process at one of its most crucial stages—the discovery of a problem.

IMPACT OF SUPERVISORY PATTERNS AND ADMINISTRATIVE METHODS

The previous sections have dealt with organizational or structural defects which impede individual contributions to the success of the firm. Most are a product of the division of labor, the formation of separate departments which evolve distinct and often incompatible subgoals for themselves and frequently fail to intermesh or complement one another's activities. There are other problems caused by inept administration.

Influence of Supervisory Style

Because of our historic emphasis on the individual and the importance of personal leadership, studies of supervisory styles have been popular for

a number of years. Early questioning of autocratic methods led many "enlightened" students and administrators to jump to the conclusion that "free" or "general" supervision was more conducive to productivity and morale. More recent studies have led to serious questioning of this equating of morale and leadership style with productivity of workers.[9] However, this question of general supervision and productivity has not been seriously extended to areas where the functions of the supervised require creativity. Assuming that innovation requires a free, uninhibited flow of activity and interaction because it relies on borrowing and analogy, observers have argued that a free intellectual atmosphere similar to that in a university is most conducive to innovative thinking.[10] Our participant observers do not completely agree with these views.

As a plant technical engineer, Franke apparently liked freedom from supervision for his technical work on the coating gauge, but he decries a lack of supervision for nontechnical activities.

> I rarely saw my supervisor except when he spent a little time to give me another assignment, or rare checks on the progress of a past assignment. Since the big technical push was in another area, I was practically unsupervised. I felt this was a considerable disadvantage because an engineering supervisor can acquaint a new man with a lot of the details about the work situation and can help him on the nontechnical problems that the engineer hasn't been successful in handling.

Absence of supervision is a disadvantage here because the subordinate feels weak without someone to strengthen him in his dealings with production supervision and other departments. Franke wants someone with leverage to whom he can go for assistance.

In the contemporary organization, supervision, little or great, does not only emanate from one's supervisor. One feels pressures (or their absence) from individuals and groups who precede or follow one's position in the flow of work. Orthodox theory again would suggest that individuals don't want pressure; they want to be left alone. But again this easy-to-accept principle in human relations is not borne out so simply by our observers. As Webber notes:

> Both I and my colleagues welcomed some work pressures from other groups around us, particularly from the production departments we were supposed to be serving. The pressures from line management give the engineers a sense of "belonging" to the production department and of their work being important enough to warrant interest. In this particular situation, line pressure usually does indicate that the IE's assignment is important. The engineers respond with

[9] Cf. Studies of the Survey Research Center, University of Michigan, Ann Arbor, Mich.: Katz, Maccaby, and Morse, *Productivity, Supervision, and Morale in an Office Situation*, 1950; Katz, Maccoby, Gurin, and Floor, *Productivity, Supervision, and Morale among Railroad Workers*, 1951; Donald Pelz, *Power and Leadership in the First-line Supervisor*, 1951.

[10] Cf. Henry Eyring, "Scientific Creativity," in Anderson, *op. cit.*, pp. 2–12, and Simon Marcson, *The Scientist in American Industry*, Industrial Relations Section, Princeton University, Princeton N.J., 1960.

extra hours of work and additional effort. Of course, the extra effort demonstrated is also related to the fact that the engineer has been in the spotlight and knows it. His conspicuousness presents an opportunity to build a reputation on something considered valuable.

Brown, in his coal-and-coke research department, was also critical of the excessive passivity of his supervisor. He observed that since the manager did nothing to relieve the rivalry between the two major subgroups in his department, his men thought him weak and ineffectual. In turn their contempt contributed to still further animosity within the department.

On the other hand, in the industrial engineering department, Webber experienced the disadvantages of excessively close supervision. The effects he felt at the time are graphically and emotionally described in this excerpt from his report:

> There is widespread discontent among staff engineers because of the detailed supervision they receive on their technical work from the manager. He has been incapable of refraining from criticizing aspects of a new idea even before the whole plan is presented or thought out. This action tends to hinder progress on all innovative ideas and to destroy others completely. Of course, one can complain that the engineers should be stronger individuals in standing up for their ideas; but excessively critical evaluation of an idea at an early stage can convince the originator it is not likely to be a fruitful course of action. This is not caused by a weakness in the engineer's personality, but is due to the natural frailty of all the facets of his idea and is incapable of defending it in the earliest stages.

The adverse effects of close supervision are not reflected in low productivity (or at least in pace or diligence) so much as in a redirection of effort to items of less importance. Franke, for example, supplied an example of wasteful redirection of effort. Unreasonable criticism of one of his drawings caused him to devote extraordinary time to redrawing it to excessive perfection—a step which did not contribute to progress on the job. In the industrial engineering division, close supervision is also reflected in emphasis on "selling," getting new jobs for yourself, and applying the esoteric mathematical techniques we have already discussed.

The key to the possible coexistence of close supervision and zest for work in the IE division, however, was found in its past record of rapid promotions. The division historically had served as a training ground for the whole company.

> There may be some correlation between closeness of supervision and opportunity for promotion. This industrial engineering division always had too many supervisors with little to do except supervise subordinate engineers. Although the engineers haven't welcomed this supervision (and indeed, most of the supervisors haven't enjoyed it), it has tended to provide rapid promotional opportunities and "paper" supervisory experience for the engineer's record. Close supervision coupled with frequent evaluation for possible promotion has prevented apathy.

Monitoring Work

Evaluating creative personnel for promotion brings us to the question of how the supervisor monitors or measures the performance of subordinates. Webber implies that his colleagues traded greater freedom in their work for closer supervision (and he implies, a greater quantity of information on who is promotable). On the other hand, Brown observed that managers in the steel company's research department were reluctant to exercise this type of supervision, although they also believed they lost something as a result: adequate information about what was going on. Parenthetically, it is interesting to note that the subordinates in the research center were apathetic, while Webber reports that he and his fellow engineers were enthusiastic and had "high morale."

Clearly the measurement process affects the quantity of initiative and originality the subordinate will demonstrate in performance. What is he rewarded for? What is he punished for? How consistent and reliable is the reward-and-punishment system?

The steel company research center's managers relied on the number of completed research reports as a measure of "how things were going." In fact, each research group had a quota. Yet the men recognized that a quota of reports was a poor control in a research environment, and that numbers alone could never reflect the "numerous technical and human variables which affect a research project."

Because they did not accept the validity of this control and believed that their managers knew little else about their work, "there was widespread apathy and resignation."

> The research center's production was measured in completed research reports, and a quota existed for each research group. That the quota system was not proving very effective was evidenced by the fact that it was only loosely adhered to. It was a poor statistical control because it failed to consider the numerous technical and human variables which affect a research project. The supervisors on all levels also used personal contacts in supervision but appeared to be torn between their need for knowledge of group working norms and their dislike for constantly looking over the shoulders of their subordinates.
>
> I had been working in the coal-and-coke division only a few days when I began to realize that seldom was anyone in a hurry to accomplish anything. . . . Laborers and technologists alike loafed and made few attempts to cover up. . . . One man liked to say that he told each new, hardworking technologist that he had six months. This meant that even the most conscientious new man would soon become aware of the needlessness and futility of working hard.
>
> The men who could ignore the working conditions, who could shut out the noisy argument or discussion going on around them and tend to their work in a routine fashion were the ones who lasted the longest and eventually were promoted. . . . They were fairly quiet individuals and rarely tried to exercise any leadership in the group. They were neither too ambitious nor too questioning. The ones who thought they could do very good work lost their interest fairly quickly.

The eventual promotion by seniority of those without demonstrated desire to be active and initiate interactions led to less than optimal supervision at lower levels.

Thus the reward system favored the plodder.

"Management by results" is a meaningless phrase for most staff work.[11] In the first place, on projects of a research nature, even the most optimistic cannot expect a very high percentage of "successful" results. Emphasis ought rather to be on evaluating and controlling progress within certain limits, so that managers may determine whether the organization system is operating properly and moving toward a successful outcome or whether a new direction should be pursued before a bad result occurs. Similarly, on more orthodox staff-service functions, results are often far removed from the staff's activity and are virtually useless as a control technique. In the steel company research center we saw that there were fears about relative status and management recognition between two groups working along similar lines. The situation was characterized by an almost complete lack of adequate monitoring. The real control appears to be a requirement for a certain number of technical reports each year.

In order to get some sort of standard, the director of the research center had made it known that each group was expected to publish each year approximately three times as many blue books (reports) as it had technologists. Our technologist confided to Ed and me that each of them had one or two things in his desk that he could make into a blue book within a week if he had to.

Some of the "irrational behavior in intergroup relations is related to the inability to monitor staff operations: for example, the efforts to look important, to get to some position, or to present an idea ahead of another group in order to keep them from "looking good."

The difficulty in monitoring performance also contributes to the confusion, which was mentioned earlier, in the relationships between staff and line in the industrial engineering department.

Industrial engineering management had historically used "savings" supposedly produced by IE projects for evaluation purposes. Over the years, however, it became apparent that these cost savings were less an indication of the individual engineer's work than merely a summation of all the changes which the production department had installed since the last IE study. Nonetheless, paper savings still are a factor in evaluating IE performance, and the individual pays substantial attention to them.

Production department evaluation of industrial engineering activities was even more nebulous. Realizing the uselessness of claimed cost savings, production supervision evaluated IE for its service contribution—estimating how much they eased the job of the manager.

Our discussion of conflicting goals indicates that IE management's evaluation on the basis of savings from innovation may directly clash with production

[11] Management by results means an absence of continuous monitoring in favor of checking "results" at the time of actual completion. The argument is that if the goals are met, the superior need not be concerned with the methods used by subordinate supervisors, thus ensuring freedom and flexibility of operation for the subordinates.

management's evaluation of the engineer's help to them in meeting operational problems, including day-to-day maintenance of work regularity. The engineer finds that he is not sure which management he is to satisfy and under what standards of evaluation.

On the one hand, IE supervision controls his pay raises and promotion in industrial engineers. On the other, engineers have normally been young people with line-management aspirations. Transfers out of the division have depended on being requested by the production departments. The lack of objective criteria for monitoring the engineer's innovative work leads him to transfer his main attention from the quality of his ideas to attempts to find out whom to please and how.

Here we see how monitoring or controls can also affect intergroup relations, not only superior-subordinate relations. How well a group that is supposed to facilitate the work of another group actually performs this function depends upon how each is being measured and rewarded.

Webber comments that his department had a structural problem, which we have already referred to: excessive numbers of personnel. Under one system of monitoring, where individual production departments were not charged for the time of engineers who worked for them, one effect was produced. But when top management changed its measures so as to charge production units for the engineer's services, quite a change in attitude and behavior occurred.

Since the division had historically been used as a training area for new technical managerial personnel, the excessive number of people was not a handicap. Effort was all too often concentrated on trivia, but the work did offer experience (and facilitated individual promotions), and since the individual production departments were not charged for IE time, they did not complain.

Plant management changed the budgeting of industrial engineering from a charge against general plant overhead to a system which charged IE hours on each assignment directly to the production department concerned. Because of this step, production supervision began to question closely the quality and contribution of IE services. Staff management began to feel that the young engineers could not develop enough ability to perform optimal engineering work in the short time they had been staying in the department. The decision was made to emphasize career opportunities in industrial engineering and to cut down on promotions out of the department in order to build up the experience level. This change had a harmful effect on employee morale. The division was filled with bright, frustrated people because of the seriously curtailed promotional opportunities.

CONCLUSION

This chapter has endeavored to summarize three case studies of departments that were supposed to be creative and to tap the initiative and ingenuity of their employees. As we have seen, their relative success and failure was a function of some rather sophisticated interrelationships between groups and between supervisors and subordinates. Simple homilies about leaving the creative person on his own, supervising by results, making one department the source of creative ideas and leaving the other

to do the routine production work—just aren't successful. They neglect the realities of organization life, the challenges posed by a division of labor which requires individuals and groups to intermesh their activities.

Individual creativity and initiative are as much functions of structural and administrative technique as of individual competence. Surely the brain power and skill of the individual is necessary, but it can be useful only in so far as management can comprehend the subtleties of organizational behavior.

The picture of the large corporation that Webber and his associates have given us is far different from one imagined by the armchair theorists. As Harlan Cleveland has told us in Chapter 2 [in *Individualism and Big Business*], the organization is no hegemony. We don't find people being swaddled by excessive security or manipulative supervisors. Excessive bureaucratization is much less of a problem than its opposite: an absence of regularity, of clarity of job assignment, or of harmonious interlocking of managerial positions. Rather than a stifling atmosphere of peace and quiet, the modern organization has a good deal of internal conflict. The individual has ample opportunity to express his individuality, but the usefulness of that expression depends upon the degree to which excessive intergroup competitiveness and conflict can be moderated. However, the creative employee who expects that the wisdom of his ideas will carry them to acceptance is being deluded. As distinct from the solitary scholar putting pen to paper and waiting for the world to read and marvel, the member of the organization must depend upon his skills for comprehending the patterning of human relationships and being able to cope with them.

Chapter 30

THE DUAL HIERARCHY IN RESEARCH*

HERBERT A. SHEPARD†

IN RECENT YEARS industry has become increasingly dependent on technological innovation as an instrument of competition. Within the individual firm, power tends to shift to those who possess the skills most needed for survival and growth. Over the past half-century, this distinction has passed from manufacturing to sales to research and development. Staff-line organization ideology which developed as a tribute to the pre-eminence of manufacturing has slowed and obscured shifts of power, has been modified to become a theory of organization applicable to the structure of sub-units of the firm, and has tended to give way to functional ideology which allows power to flow more freely in response to changing environmental demands.

The shift of power to research and development has been hampered by staff-line theory which defined it as an advisory service rather than an innovative force or a higher center of intelligence. A more ancient ideology, which developed as a tribute to the rise of the bourgeoisie, based on the classical economic theory, also hampered the shift of power by defining monopoly to support it and survive the period until another cushion can be provided by innovation; the firm must have a long time perspective. The shift of power has been further hampered by the ideology of science which denies the relevance of social power to scientific work, and by the social norms of physical science which exclude and deny the validity of certain types of knowledge and skill required for innovation—political, economic, organizational, social, psychological, cultural, etc.

The relatively weak position of the research and development laboratories of many firms is reflected in their acceptance of the organizational traditions of the rest of the firm, even though these traditions are adapted to the requirements of production rather than research. Until recently, few industrial laboratories have abandoned the organizational model suited for efficient repetition of operations and sought a model more in

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keeping with the requirements of innovation, or the mobilization of intelligence.

It is by no means certain that solutions to problems of coordination, control, evaluation, program formulation, personnel maintenance, decision-making and the provision of administrative services can be found which do not in themselves interfere with the mobilization of intelligence. However, the process which produces these problems also produces a managerial class which is concerned with them and with the mobilization of intelligence. Research managements are experimenting with new organizational arrangements intended to resolve the dilemmas of laboratory organization which confront them.

The problems of research and development organization which confront research managements are not necessarily defined by them in the terms this paper uses. Problem definition is influenced by the situation of the definer. The experience of a research manager is more likely to be organized as follows. As a technical organization grows in size, problems of coordination, control, evaluation, program formulation, personnel maintenance, decision-making and the provision of administrative services become more complex and burdensome. A managerial class develops which is concerned primarily with these matters. The class is hierarchically differentiated as is the custom in most large scale organizations. Since its responsibility entails control over the activities of scientists and engineers, it is logical that technical competence be one criterion for entry into the class. However, entering the managerial class removes the technical man from direct participation in technical work, and he comes to devote himself to many matters not recognized as technical. When a good scientist is made a manager, a good scientist is lost. But promotion to management is the reward for competence in scientific work. Hence the laboratory becomes a school for making nonscientists of its scientists.

This anomaly having been recognized, the intelligent managerial action is to seek a corrective device, a means of preventing the undesirable outcome. But getting rid of an undesired consequence without at the same time destroying the institution that produces it, or without producing other equally undesirable consequences in its place, is rarely an easy undertaking. Research managements have experimented with a number of methods and approaches. Two of these will be described and discussed in the following pages.

In simplest terms, the problem appears to be to find a way of rewarding scientists for good scientific performance without removing them from scientific work. One approach to this problem emphasizes the concept of technical direction. For example, administrative assistants may be provided whose duties are to perform nontechnical functions for research supervisors and managers, so that the latter may spend their time directing technical activities, and thus continue to make a scientific contribution. At

the same time, it is felt that by a high degree of technical competence represented in the laboratory management, better results will be assured.

Emphasis on the concept of technical direction produces new problems. Some commonly experienced difficulties are as follows:

1. *The search for inspirational leaders.* The skills involved in technical leadership are not well understood, nor do they appear to be readily transferable. Some scientists are gifted in this respect, but the demand appears to exceed the supply.

2. *The productive scientist who is not acceptable to others.* When there is evidence that a scientist lacks the aptitude for "technical direction," there is no alternative way to reward him for excellent scientific performance.

3. *The morale of administrative assistants.* If the administrative assistant is a scientist by training, he is likely to regard his administrative post as evidence of failure as a scientist. If he were a successful scientist, he would be a technical director, or at least a practicing research worker. If he is a non-scientist, he is likely to sense disparagement of his skills on the part of the scientific staff. In either case, a satisfying career is difficult to work out.

4. *The effect of a hierarchy of technical directors.* Emphasis on the concept of technical direction implies that technical directors have a responsibility for technical decisions. Inspirational technical leadership is one side of the coin; control is the other. The inspirational leader need not be a controlling figure, but it is difficult to avoid producing a system of authoritarian control where there is a hierarchy of technical directors.

The most serious criticism of emphasis on the concept of technical direction is that it is wasteful of the laboratory's scientific resources. The exercise of technical judgment by a technical director may be regarded as top company management's way of decreasing the likelihood of erroneous technical decisions, since top management must usually accept the technical aspects of laboratory reports on faith. But the technical director's knowledge is inevitably inferior to the combined resources of his scientific staff. Hence his contribution is likely to be redundant at best, or negative at worst. The greater his knowledge, the more likely is this danger to be overlooked, and the more likely is his staff to be dependent on his scientific resources rather than their own. The danger that poor decisions may be made does not derive from the danger that the director may be technically incompetent, but from the danger that he may not know how to ensure that the scientific intelligence of his staff is brought to bear on problems.

Some of the difficulties associated with emphasis on the concept of technical direction have led research managements to experiment with another approach to the problem of mobilizing scientific intelligence. The approach is called the "dual hierarchy" or the "technical ladder"

method. It springs partly from the same problem definition as do the experiments which emphasize the concept of technical direction: namely, that the best scientists are lost when they are rewarded by being made managers. A companion problem, namely, that some able scientists are in their own opinion or in the opinion of their associates or of management, singularly unfitted for carrying managerial responsibilities, also motivates the dual ladder approach. It is intended to make the practice of research as rewarding or attractive as the practice of management.

The dual ladder approach is roughly as follows. The laboratory, let us assume, is divided into departments, each with a department head. Each department is divided into sections under section heads, and each section is further subdivided into work groups of scientists and engineers, under group heads. This constitutes the managerial ladder. Now another set of positions is established, roughly paralleling the department head, section head, and group head positions in terms of salary, job luxuries, and freedom of decision on work matters. Terms are invented to label these positions, for example, Research Associate, Senior Research Associate, and Scientific Advisor, in ascending order. This constitutes the technical ladder. Research Associates report to Section Heads, Senior Research Associates to Department Heads, and Scientific Advisors to the chief executive of the laboratory. No managerial responsibilities encumber the freedom of persons occupying these positions. Thus it becomes possible to give recognition and reward to scientists who do outstanding work by promoting them in the technical ladder, at the same time providing them with opportunity to continue in scientific work. The scientist has two ways up in the organization.

There are several variations of the above approach, but they share in common the idea of acquiring and maintaining productive scientists in scientific work, by rewarding them with prestige, freedom and job luxuries (special parking spaces, comfortable offices, secretaries, private laboratories, etc.). As with the approach previously discussed, certain difficulties are associated with the dual ladder method. Some common ones are as follows:

1. *Role definition for the technical ladder position.* Usually, the keynote of upward movement in the technical ladder is meant to be freedom in research. According to scientific mythology, such freedom is highly valued. In the context of the industrial laboratory, however, it may not be desired by the scientist, from any of several causes. For some, such freedom is loneliness, rejection, the feeling of not belonging. It is valued highly only by relatively autonomous people. But frequently the technical ladder position seems appropriate for those members of the staff who are regarded as "prima donnas," that is, for people who are already experiencing rejection by their associates. The reward of a technical ladder position confirms their worst suspicions. A dropping off of productivity is sometimes noted after the appointment to a technical ladder position, and

may be attributed to resting on his laurels, or to inadequate supervision. But it may also be his reaction to discovering that the means he has been using to impress others and gain recognition and acceptance have failed, and he has been formally isolated from the rest of the staff.

In some dual ladder systems, the incumbent is expected to serve as a consultant to others in matters affecting his specialty. The provision of consultant aid requires a high degree of skill, sensitivity to the feelings and needs of others, ability to help without damaging the other's self-esteem. But the appointment to a technical ladder position is rarely accompanied by training in consulting methods; moreover, there is a tendency to appoint scientists who show skill in "human relations" to positions in the managerial ladder. Once again, the technical ladder incumbent is likely to experience rejection and to react in ways which are not serviceable to himself or the organization.

2. *The technical ladder position as reward rather than opportunity.* When the emphasis in promoting a scientist to a technical ladder position is reward for past service, certain difficulties may be encountered. Scientific mythology tells us that the scientist is motivated by curiosity. The picture is usually presented of the scientist as possessed by a burning desire to undertake some favorite projects which he has been prevented from doing by the requirements of his present job. Appointment to a technical ladder position may succeed primarily in revealing the scientist to himself and his associates as lacking in the independence and commitment which he was formerly free to pretend.

3. *The technical ladder position as a shelf.* The reward aspect of appointment to a technical ladder position, together with its capacity for isolating the individual from the rest of the organization, makes it a convenient shelf for senior staff members whose managerial or technical skills are outmoded or wanting, but who occupy influential positions. To the scientific staff, such appointments are an admission by management that "promotion" to a technical ladder position is a punishment rather than a reward.

4. *The technical ladder position as an ambiguous status symbol.* Within his profession, there is no universality of title meaning for the industrial scientist, as there is for the academic scientist. While ambiguity may occasionally serve purposes of face-saving or making an impression, it is a source of stress when the scientist is trying to identify himself relative to other members of his profession. The problem of identification may be more stressful in the non-scientific community of neighbors and family, where status and worth are identified by widely recognized labels and by such criteria as the size of organization the individual commands.

5. *The technical ladder position as proof of inadequacy.* The scientist who is gifted in his ability to inspire and lead others as well as in his science belongs logically in the managerial ladder. The technical ladder is usually reserved for competent and brilliant scientists who are regarded as

lacking in managerial potential. In our society, leadership skills and leadership positions are highly valued. The technical ladder incumbent is only half a man. Under these circumstances, promotion to a technical ladder position is a dubious honor—it is as much stigma as reward.

6. *Promotion in the technical ladder as mobility up and out.* The characteristics that make the technical ladder a convenient shelf make it unattractive to many scientists. It is peripheral rather than central. It removes the scientist from the mainstream of organization. It gives freedom in place of power, but power is needed to remain free. Moreover, as noted above, appointment to a technical ladder position is in many laboratories a judgment that the incumbent is unfit to exercise power. This permanent exclusion from the mainstream of organization means that the technical ceiling, in terms of income and prestige, is low in comparison with the managerial ceiling. A combination of managerial and technological skill is highly valued in modern corporations. The research scientist or engineer may well aspire to a position in top management if he begins to move up the managerial ladder; but a step up the technical ladder is towards a point of no future and no return.

7. *A shortage of rungs in the technical ladder.* In some laboratories, the growth of a technical ladder has been topsy-like. In these cases, it often happens that there are only one or two prestige and income categories, with the result that a scientific career is out of the reach of scientists who are only competent but not brilliant, or the alternative result that the ceiling for advancement in the technical ladder is so low as to be unattractive to ambitious scientists.

8. *Technical ladder positions are less secure.* The productivity of the scientist in a technical ladder position is more easily assessed, or at least more open for inspection, than the productivity of a manager. Especially if he is relatively isolated from the rest of the staff, doing independent work, there is no difficulty in fixing the responsibility on him if his results do not come up to management's expectations. In evaluating a supervisor or manager, there are many considerations and many areas of irreducible uncertainty. Unless the scientist in the technical ladder has been able to take a valued consultant role, however, his situation is uncomfortably simple. Moreover, much more is expected of the scientist in a technical ladder position than performance of the routine tasks that comprise the bulk of scientific work. He is expected to be creative. But persons in the managerial ladder may be highly regarded for providing leadership to groups performing work that is largely routine.

That the dual ladder is a sensitive system, subject to breakdown from many causes, does not demonstrate that it is unworkable, but that to thrive, it requires conditions which are hard to establish and maintain. Some prescriptions and proscriptions can be inferred from the foregoing list of pitfalls.

First, the opportunity rather than the reward aspect of the technical

ladder position should be stressed. It should be determined whether the candidate does have a burning desire to do some independent work. This consideration suggests that the technical ladder position should be a temporary, rather than a permanent commitment.

Second, it suggests that research workers who are primarily oriented to success in their profession rather than in the company are the most appropriate candidates for technical ladder positions. If the scientist regards the laboratory simply as a convenient place to do his work, he is more likely to feel liberated than isolated in the technical ladder.

Third, it suggests that training in consulting skills and the provision of consulting opportunities are important integrating elements. The scientist in the technical ladder can still be an influential member of the organization.

Fourth, it suggests that great care must be taken in selection of candidates for technical ladder positions, since the prestige of the technical ladder rests only on the willingness of good scientists to accept positions in it. Relegation of deadwood to the technical ladder destroys its potential value.

But the most important issue is the problem of organizational influence or power. It is interesting to note that in the British Scientific Civil Service, the assumption appears to be made that anyone can manage, but it requires rare talent to do research. There, the equivalent of our technical ladder positions are sought after and high scientific rank achieved largely through managerial advances is regarded as less valid than high rank achieved by climbing the technical ladder. In our own country, however, it is doubtful that we could ever support the myth that science is a more important or more valid activity than management with enough evidence for it to become reality for scientists. Some laboratories have been successful in removing the stigmata from technical ladder positions, by freely transferring scientists from one ladder to the other in accord with research requirements, research workers' inclinations, and career considerations.

More fundamentally, however, it may be desirable to re-evaluate the functions of members of the managerial hierarchy. Emphasis on control and direction of a professional group seems almost a contradiction in terms. In the professional mythology, a research manager can scarcely accept responsibility for the work of his staff; they have that responsibility. But professional mythology is no match for the realities of American life. For many young scientists and engineers, the practice of their professions is primarily a means of getting a management position.

The denial of desire for power in the official version of the scientific career is a source of confusion for the scientist, rather than a valid basis for a calling. The young scientist has not acquired the self-image or the skills required for sharing responsibility. Dependency pays off in the laboratory; it makes a cooperative subordinate, suitable for promotion to a position of

responsibility. Thus authority and dependency become the formula for laboratory organization, and a technical ladder must be tacked on to free some scientists for creative work. There is needed a new concept of research management, which helps nominally professional workers to acquire the self-image and skills required before they can accept professional responsibility. Unless scientists are able to participate responsibly in decision-making, to use each other's resources, to engage in mutual evaluation, and to work autonomously, a controlling, centralized management is necessary.

SECTION V

Complex Case Series

Chapter 31

THE ATLAS CORPORATION (A)*

THE ATLAS CORPORATION[1] had been for a number of years one of the largest of several manufacturers of specialized machinery used by the automobile industry. More recently the company's major product line was being expanded by a diversification program focused on other industries using custom built machinery requiring intricately engineered mechanisms.

Since the company started in business in the early 1920's, Atlas had dominated its primary market by building up and jealously guarding essential technical skills and patents that discouraged any competitor from rivaling its constantly growing superiority. Until recent changes in the market, Atlas had enjoyed 30 years of uninterrupted growth save for a minor reversal during the depression period of the early 1930's. Beginning in 1950, however, other manufacturers, using knowledge acquired while producing machinery for defense plants during World War II and the Korean war, began to move into Atlas' market and seriously threatened the company's position. This threat, becoming more pronounced in the last three years, was having far-reaching effects on Atlas by 1960, and the corporation, in response to this threat, was beginning to react to management's efforts to reorganize, decentralize, and modernize their operations.

The company had always engaged in many activities designed to establish friendly relationships with employees and the surrounding community. The main plant, corporate offices, and research facilities were situated in a small suburb of a large city where a large share of the automobile manufacturing effort was located. The town had always been economically dependent on Atlas, and the company had accepted this responsibility by providing steady employment for its work force even through the depression. Consequently, employees felt a high degree of loyalty to Atlas, and it was not uncommon for two and three generations of one family to be working at one or another of the plants or offices at the same time. After an employee had been with the company for ten years or more, it was generally accepted that he would be kept on until retirement. Discharges and layoffs were uncommon.

Though the corporation had given up many of its past paternalistic activities in the preceding several years, many of the last vestiges of the

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[1] All names disguised.

close interrelationship between the community and company still existed in the summer of 1960. Many of the community's social gatherings, for instance, were strongly supported, if not actually sponsored, by Atlas. Moreover, most of the firm's personnel confined their off-the-job social contacts to fellow Atlas employees, and most frequently, chose only those within the same section or group. The Atlas Athletic League, an institution of long standing, still performed many of the same functions normally provided by the Y.M.C.A. and country clubs in similar communities, organizing sports competition and initiating many informal social groupings.

Recent Changes

The more competitive situation being experienced in the automobile machinery market had recently forced Atlas to diversify activities into other industries. At the same time, and as a consequence of this, management initiated a reorganization of the company which was affecting every aspect of the internal structure.

Many of the key management positions were being handled by men new to their jobs. Most of the new supervisory talent was being obtained from within the organization, and most of these men appeared to be willing to test out new ideas in an attempt to revitalize the organization. Central to the reorganization plan was an increased emphasis on decentralization. Management's goal was to create autonomous units, each operating as a separate business. Even functional parts of the organization such as the casting department or welding department were to have their own separate budgets and monthly profit and loss statements. It was understood throughout the company that in the near future management would expect each department and section to be in competition with outside organizations for the "business" of another division, section, or group within the company. The research organization, for instance, would have to provide its services at a "cost" that would be consistent with the outside going rate for research efforts. Thus, intercompany negotiations would be made on a bid basis, which would presumably provide an incentive to prune down the excessive indirect expenses that had accumulated during the period when the company had dominated its market.

As one of Atlas' senior officers expressed it, the general complexion of the company was rapidly changing from a lethargic organization, grown "fat" from lack of incentive, to a trim fighter ready to meet the new competitive situation. While much of the reorganization of the higher echelons had been completed, the personnel at lower levels in the various departments had just begun to feel the effects of these and other changes.

The Research Division

Largely responsible for the company's success in the past and heavily relied upon to carry it through the present transition was Atlas' extensive

research division. Housed in a two-story building several blocks from the main manufacturing plant, the division was one of the early industrial laboratories in the United States. It was now manned by a complement of some 1,000 employees, compared with a staff of 15 persons when research was first recognized as a unique effort at Atlas. (See Exhibit 1 for a partial organization chart of the research division.)

The division's main responsibility was the development of new machine designs for the automobile industry. Although some "basic work" was carried on in advanced mechanism design, the majority of the division's personnel were engaged in the various operations required to bring a new machine idea to commercialization.

The man primarily responsible for reviving an aggressive research effort was Mr. Gerald Ives, Atlas' research laboratory director. Ives had come to the research organization after graduating from a mechanical engineering course in the late 1930's.[2] Upon his shoulders rested the major share of the burden of improving the efficiency of the research organization as it worked toward two major goals; development of new specialized machinery for the automobile industry, and, more importantly, preparation of the company for diversification by developing radically new machinery for other markets.

Mr. Ives reported directly to a vice president of research. In 1959, the latter office had dramatically changed hands when an executive who had been a member of the "old regime" retired and a younger man who had been groomed for the office by the new administration was promoted. While the older officer had taken an active and participative interest in research-administration, the current vice president encouraged Mr. Ives to handle the majority of the day-to-day decisions, reserving only those concerning basic policy for himself. Mr. Ives had initiated many policies that he believed were long overdue if the research organization were to be transformed into an efficiently operating unit.

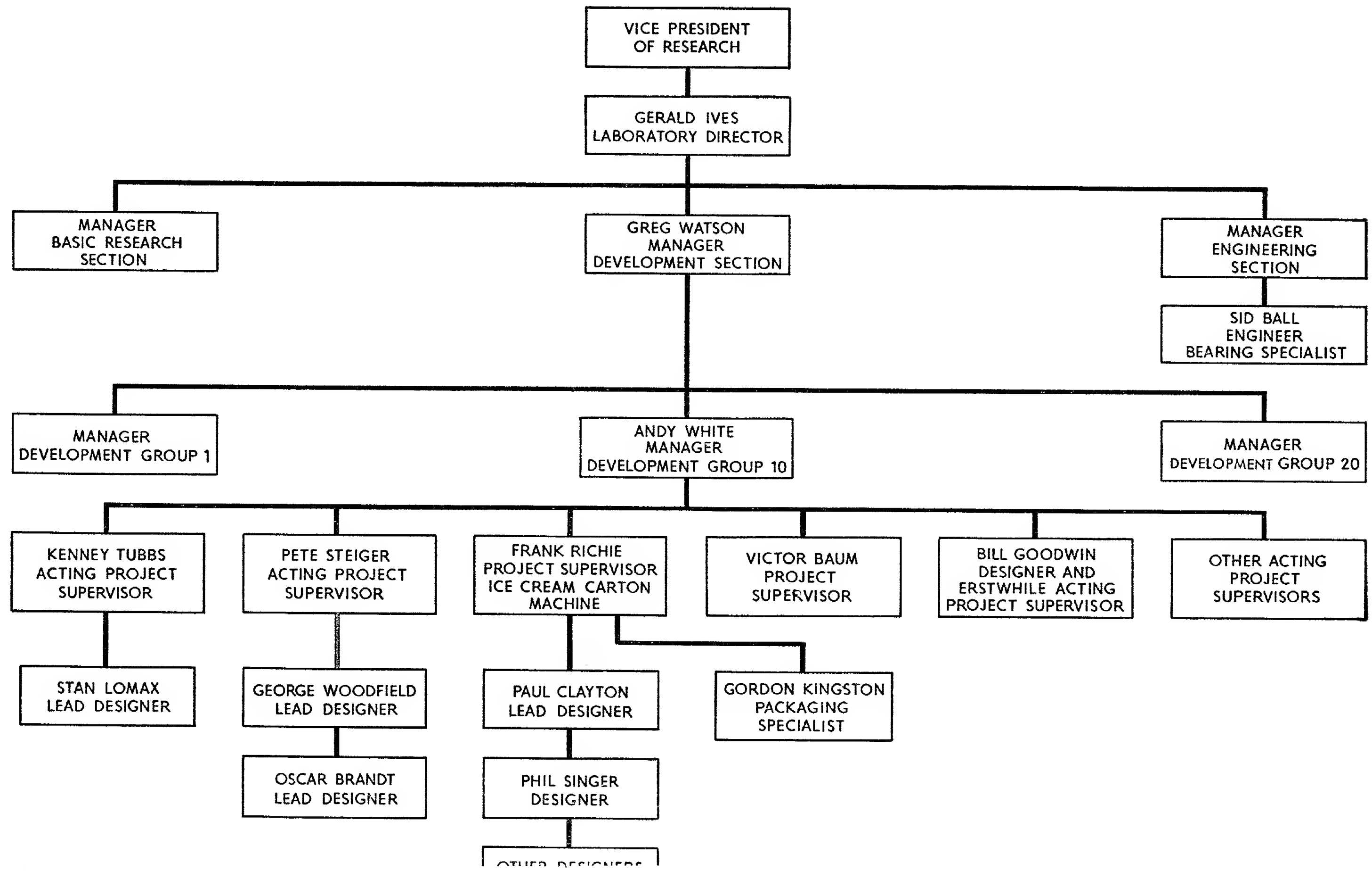
During more than 20 years in the job, the former vice president had behaved in ways which were typical of company management during that period. He had personally employed most of the older personnel within the organization and, with a few exceptions, was on a first-name basis with every employee hired since 1955. His subordinates both feared and revered him. He was well known for frequent walks through the laboratory, during the course of which he habitually stopped to talk with researchers and inquired into their activities. He had an exceptional memory for details and was able to keep track of progress on almost every project. During his "regime," intermediate levels of research management made few decisions. He exerted intimate, personal control over nearly every aspect of research activity, and quite frequently the machine designers would consult him directly concerning decisions involving even minor alterations in previously approved plans.

[2] During 20 years of service in the research division, he had moved slowly up the ladder of middle management and had been promoted to laboratory director in 1958.

Exhibit 1

ATLAS CORPORATION

PARTIAL ORGANIZATION CHART OF THE RESEARCH DIVISION



When Gerald Ives was promoted to the job of laboratory director, his superior was close to retirement and was becoming visibly less active in day-to-day affairs. Ives found that he was able to begin the job of methodically revamping and modernizing the research activity. In so doing, he tried to apply the latest techniques of research management. He frequently attended management seminars and conferences where new concepts were discussed and believed these new ideas were helpful to him. He encouraged his subordinates to do likewise, granting leaves of absence for those who desired to participate in such activities.

Mr. Ives found little time for periodic visits to the working area, leaving this function to his immediate subordinates or their representatives. His days were filled with conferences and meetings where he negotiated the various changes he was trying to initiate. Details relating to new product lines were taking an increasingly larger portion of his time. As he saw it, it was his job to outline the broad policies affecting work methods, supervisory relationships, and product policy and then allow the middle-management group to implement its own detailed program.

Over the years, Ives had become increasingly disturbed by what he called the "lethargic attitude" displayed by research personnel. He felt his major responsibility was to revive the spirit of the research division while at the same time planning for better direction of the activities of all those below him. A feeling for this drive and determination can be sensed from the topic headings Mr. Ives used in a speech prepared for a group of visiting manufacturing supervisors.

The research division:

1. Obsolete ways of operating require a better thinking system.
2. Less time and money should be used to accomplish our work.
3. We want less design costs.
4. We need better products.
5. We need more diversification.
6. Every darn dollar is important; therefore we must account for every direct and indirect expense. Proper expenditures, however, are a must for the future.
7. The time is short.
8. We must eliminate our 40 years of . . . .
9. We must separate the men from the boys.
10. We must upgrade those who deserve promotion and eliminate those who don't.
11. We must bring in good outside talent so we can cross-fertilize our knowledge.
12. We've got to get out and see what others are doing.
13. To change our organization to fit the times is a must.
14. Responsibility has got to be passed down.
15. We must cooperate as a team, but at the same time realize healthy argument is beneficial.
16. More science and engineering has got to be applied earlier in the machine development sequence.

The Development Section

This case will focus primarily on the influence of the innovations and changes initiated by Mr. Ives on the development section of the research division and examine particularly the effects upon one particular group within this organization. (Development Group 10—see Exhibit 1.)

The development section, which was in 1960 by far the largest in terms of personnel and budget relative to the other two sections, had historically been the nucleus around which the entire research effort had grown. Many of the older employees presently working in other areas of the laboratory had once worked in the development section, and the entire engineering section had once been a group attached to the development section.

Greg Watson, the section manager, had only within the last year been given formal supervisory responsibility, although he had been acting in that capacity for over two years. He, like the new vice president of research, had taken over from an "old regime" man who had held the office for a period of over 20 years. Mr. Watson's qualifications for the position of section manager included 22 years' service in the section and seven years as manager of Development Group 10.

Historical Background of the Developmental Team

A developmental research effort at Atlas began to emerge in the early 1920's when management authorized a group of 15 machinists, who had previously displayed a talent for creating ingenious machine designs, to design and develop new machines for the company to manufacture. The organization of the group was very informal. In fact, most of the men worked in their homes and brought in their work only when the machines were completed. Little supervision was exerted and most of the men chose their own projects.

This type of operation was not entirely unreasonable, since most of the "inventors" had previously had considerable experience in automobile factories as machine operators or repairmen and thus were well qualified to decide what machines were needed by the automobile industry.

The inventors rapidly acquired a great deal of status within the company, since the organization as a whole was so dependent on their technical and forecasting talents. The position was looked on with much respect, especially by factory personnel and personnel working in the research organization.

Each inventor who did not work at home was given a separate room or cubicle at the main manufacturing plant and was assisted by two subordinates, a designer to draw up his ideas on paper, and a machinist who made parts for the first working model. The inventors almost literally reigned as "kings" in the cubicles and were given many privileges not

accorded the designers or machinists, such as smoking on the job and unscheduled working hours.

It was not until 1938 that the "kingdoms" were dissolved in favor of a more centralized organization where more control could be exerted over the group by management. Many of the inventors and their subordinates had become specialists in the design of one particular type of machine or a particular machine design. Coincident with demands for more diversified types of machinery, this lack of flexibility in the research effort was intensified by the reluctance of the inventors, designers, and machinists to work on projects other than those which were of particular interest to them.

With the completion of a new research building in 1938, the division was reorganized and departments were created. Designers were elevated formally to equal status with the inventors. In actual fact, some inventors refused to alter their work habits and continued to work in their separate rooms or at home. (In June, 1960, two of these inventors were still active, one working in his home and the other in a separate room at the research facility.)

The centralized organization was planned to allow the group managers to switch personnel from project to project more easily in order to meet demands for diverse types of complex machinery. The new organization was based on project teams made up of designers, inventors, and machinists who were to work on various machine design projects as directed by the group manager. It was thought that, from the ranks of designers and inventors, key personnel could be chosen to help direct the technical efforts of the teams under the group managers. Initially many of these "team leaders" were inventors, but it soon became apparent that their lack of formal technical training and long ingrained work habits made them ill-suited for leadership positions. Accordingly, as the inventors were retired or died, designers, who usually had more formal training in engineering, were given the opportunity of becoming team leaders. The designers therefore gradually replaced the inventors as the esteemed group within the research organization. Through the years, the title of the team leader was gradually changed to that of lead or head designer.

The Designers

Typically, the project teams would contain three to six designers. One of them, usually the senior employee, carried the title of "head designer" and would be responsible for the overall technical design of the machine. He would "farm out" the various components of the total design to the remaining team members and supervise their activities.

Only after successfully completing several machine designs while in the position of head designer could an individual obtain the title of "lead designer." The individuals holding this rank were usually given the more

complex projects. In many cases they would be given complete charge of a project, supervising not only the technical aspects, but also the "economic" responsibility, normally handled by the group manager.

The majority of designers in Group 10 held Bachelor of Science degrees in engineering, or at the very least, had two years of training in a drafting school followed by many more years apprenticing under more experienced colleagues. While no formal criteria for differentiating various grades of designers had been announced by management, a designer was required to work on a well-understood succession of increasingly more complex design problems before he reached the privileged rank of head designer or lead designer.

Both management and other designers appeared to judge a designer's performance according to three criteria:

1. Seniority.
2. Number of successful machines worked on.
3. Number of patents held.

In actual fact, since the system of awarding patents had been revised some years earlier, the number of patents held hardly reflected an individual's personal achievement. In earlier years all patents were awarded to the head designer of a team working on a particular machine, whereas in recent years all team members shared the patents which came out of the work of the team. In spite of this paradox, patents held were regarded as indications of status within the group.

Most of the designers in Group 10 had been with the development section for over ten years, while the majority of the head and lead designers (ten men held these titles) had accumulated 25 or more years of service. As a result, most of the designers had worked on projects with one another over the years and the group as a whole was closely knit with a well-defined although informal status hierarchy.

Andy White, Manager of Group 10

Andy White had been elevated to the manager's position within the last year, after acting as the group's assistant manager for some two years. The former head of the group had been transferred into the sales organization. Andy had been with Atlas for over 15 years and in Group 10 over eight, starting in the latter organization as an apprentice designer. In 1955, Greg Watson, then the manager of Group 10, realizing White's leadership potential, gave him the choice of either completing his college education (liberal arts) and thereby qualifying for management advancement, or training intensively as a designer. Andy chose the former and was eventually rewarded with the manager's position.

Mr. White's hold on Groups 10's reins was none too firm, although his ability to manage the organization effectively had improved in the last six

or seven months. Paradoxically, much of his trouble stemmed from his previous close associations with the group's designers. Since he had been trained by them in his earlier years with the organization, some of the senior men resented his elevation to a superior position when, to their way of thinking he had not even approached the design sophistication attained by those who had continued in the designing function. Several of his erstwhile close friends within the group avoided any social contact with him. As a result he had remained quite aloof from his former designer colleagues since his assumption of supervisory responsibilities, preferrring to conduct his necessary business with them in the privacy of his office. These sessions were quite brief and usually consisted of terse comments about the work in progress. On occasion he would chat briefly with some of the designers, but for the most part his only informal interaction with the group consisted of silently keeping score for a traditional lunch-time hearts game. He had been one of the founding members of this Group 10 institution, actively participating in the game during the time he was a designer.

A year after White's promotion, Greg Watson was still helping him a great deal with major decisions having to do with the operation of Group 10, although Andy had recently exhibited some willingness to make some decisions on his own. Much to Watson's displeasure, Andy had maintained a habit of consulting the former in almost every major move within Group 10, refusing to assume complete responsibility on his own initiative. As a result, Watson had a hand in many of the promotions and project assignments within the group, making use in these instances of his intimate knowledge of the qualifications of the individual designers based on long service as both a designer and manager within Group 10.

Mr. Ives' Plans for the Development Section

Gerald Ives believed that the successful implementation of three basic, broad-scale changes would go a long way toward placing the research division in a position to contribute in full measure to Atlas' effort to compete successfully in the tougher market situation facing the corporation in the 1960's. One of these changes was initiated by top management and was intended to be company-wide. The other two were initiated by Ives to meet problems peculiar to the research division. In brief, these changes, all of which had been put into effect since Ives' promotion in 1958, were:

1. Institution of a project management system in the development section.
2. Organization of the engineering section as a separate unit and attempts to change the machine design process in ways which would make better use of engineering skills.
3. Introduction of budget controls and establishment of "cost centers" as part of a company-wide policy to do a better job on costs.

In the two years between 1958 and 1960, Ives felt that some progress had been made, but he was far from satisfied with the results to date of efforts to implement the three new policies. The case writer summarized the situation as it existed in June 1960 as follows:

The Project Management System

In 1958, having just been given more freedom to reorganize the research effort, Mr. Ives reasoned that part of the organization's inefficiency could be attributed to the highly centralized nature of the decision-making process. He felt that this meant that the company was not taking full advantage of the knowledge that existed at the working level. Moreover, he had observed that many of the research organization's most knowledgeable and experienced personnel had been leaving the company, usually complaining of antiquated procedures and questionable management decisions.

Originally, Ives conceived plans which called for the abolishment of the development section in favor of separate project teams, each headed by a supervisor reporting to a group manager who in turn would answer to himself. His proposal was met with mixed attitudes by the section managers. Greg Watson, the developmental section head, voiced substantial disagreement. He argued that the lead and head designers were presently, in effect, project supervisors, and that any change would only further disrupt an already smoothly functioning program. He thought the designers incapable of handling any more responsibility beyond the technical and design duties they already had.

Eventually a modified system was instituted, however, over Watson's objections. Compared to the original plan, it involved very little change. The organization was to remain the same as before save for the addition of an extra person, a project manager, to the traditional project team. Previously, Watson and Andy White had handled each project's "economic" responsibility. Under the new plan, the project supervisor would assume these duties along with the overall supervision of team personnel. The so-called "economic duties" encompassed such activities as preparing manpower budgets, coordinating market tests, figuring economic "pay-back" and reporting machine progress while facilitating liaisons with other sections or groups within Atlas.

Ives was extremely anxious to implement the new system immediately and despite Watson's objections, wrote and published a project supervisor's manual. Distribution of this manual was limited to management personnel. The manual described, in considerable detail, the duties of the project supervisor and outlined his responsibilities and those of his superiors under the new system.

In effect, the project manager system made each project team an autonomous organization. The project manager was supposed to operate

his "enterprise" in his own way, but it was his responsibility to see to it that he would be able to (hypothetically) "sell" his completed machine design to Atlas on the basis of good design and low cost. While he was not expected to supervise all the technical aspects of the machine design process (this function still remaining with head or lead designers), he was to be in a position to question intelligently the technical progress and recommend changes which would "in his judgment contribute to a more marketable product."

Although the administrative policy necessary for its implementation had been in effect for over two years, the actual functioning of the project management system was, in June, 1960, sporadic. Within Group 10 only two project supervisors had been appointed. These two individuals were both dividing their time between several project teams, giving only marginal effort to each. Greg Watson attributed this lack of progress to difficulties he had had finding individuals who were competent enough to handle such responsible positions. The section manager cited the fact that both the present project supervisors, Frank Richie and Victor Baum, had come from the engineering section, making the transfer only after he had enticed them with higher salaries and wider advancement opportunities.

As an interim solution, Watson had made several of the younger designers "acting project managers." These men, new to the company, were acting in a dual role. They worked as designers while at the same time handling some of the economic duties normally associated with project management. The acting project managers represented their teams in conferences with Andy White, Watson, or Ives. Some of them, having held the "acting" position for several months, alternated between two or more projects, taking care of the administrative duties in each.

The case writer was interested in the way men were appointed to the acting positions. There was usually no announcement or general notification. Greg Watson or Andy White selected a potential candidate from among the new designers and asked him to write up progress reports on the project to which he was assigned. Henceforth, if he "panned out," he was given more duties associated with project management. In many instances the candidate would, for all practical purposes, assume full responsibility for the project. The actual title of "acting project manager" was never used, however, until Greg Watson had approved of a man's appointment, at which time White would inform the individual of his selection. Frequently, the official designation would go unannounced until White or Watson would accidentally use the title before one of the other team members.

Several junior designers in Group 10 had "tried out" for acting project manager positions but for various reasons had failed to measure up to White's or Watson's standards. In these cases the administrative duties were quietly withdrawn, and the individual resumed full-time designing responsibility. Typically, the reasons for the failure would be stated by

Watson as "inability to work with the designers" or, more frequently, "failure to mobilize the project toward a fast completion."

Another criterion Watson employed in judging the qualifications of a candidate for a project management position was his ability to "handle personal relationships smoothly." The recent interest in product line diversification meant that Atlas personnel were frequently designing new machinery as a joint venture with another company already established in the new market. Under these circumstances, in Watson's opinion, it was imperative that the project manager be adept at negotiating business in unfamiliar terms with unfamiliar people.

By June, 1960, the number of acting and fully recognized project managers totaled only eight, while the number of active projects exceeded that number by four times. Andy White had appointed himself and his assistant, Ralph Allston, as project managers for six or seven projects. The remainder were divided up among the acting and full project managers, the greater load being handled by the more experienced individuals. Actually, White found it convenient to supervise projects through the various project supervisors. Since his design background did not allow him actively to criticize the designer's work, most of his dealings with the teams concerned economic or administrative details. Moreover, many of the older designers still viewed him more as a colleague than as a superior, and his directions to them were frequently not taken very seriously.

Cost Controls and Budgets

While Gerald Ives felt that the project management system was a first step in bringing home to research division personnel the importance of cost considerations, he recognized that the past history of the company and the division made it difficult to convince the long-service personnel particularly that costs had become an important element in evaluating overall performance.

During the years when Atlas enjoyed competitive dominance in the industry, cost controls were of little concern to company management. Atlas machines commanded premium prices due to their technical superiority, and "margins were fat." The research division had never been called on to do more than ask for what funds it needed and report how the money had been spent. With the tightening of competition, however, the situation had changed drastically. Top management was pushing cost control throughout the organization and Ives was anxious to meet its demands that the research division demonstrate its ability to "compete" with outside research organizations for company funds spent on the R & D activity.

The institution of individual enterprises (projects) which were supposed to be able to "sell" their machine designs to the company promoted,

in Ives' estimation, a concern for money matters that could not be achieved in any other way. At the same time, it was clear that "the proper allocation of money for the best return" was a complex goal that involved a number of difficult problems.

These problems were the particular concern of Greg Watson. He had determined, for instance, that approximately one third of the development section's budget was spent on salaries. It was therefore imperative, in his estimation, to see to it that every individual contributed to the research effort somewhat in proportion to his salary. While evaluatory techniques for measuring individual research productivity were obviously extremely difficult to apply, Watson had devised a system for measuring the performance of project teams by using as criteria the man-hours expended on previously completed projects. There appeared to be similarities in the time required for designing various size mechanisms which did not vary markedly from one machine to another. Depending on the number of men assigned to a project and the degree of complexity of the machine to be designed, the scheduled completion date could be reasonably estimated. Watson kept a progress record of all projects operating in his section and frequently consulted the chart on which the record was kept in order to spot which teams were falling behind schedule. He made use of this knowledge during his periodic visits to the design area, during the course of which he would seek out the managers of delinquent projects and request an explanation for lack of progress or delays.

If one of the Group 10 project teams fell behind its schedule, Andy White would usually ask the group's acting project supervisor for an explanation. The two would first go over the project budget for possible administrative mistakes, and if none were to be found, a report detailing possible causes for the delay would be jointly submitted to Greg Watson. In many instances Watson already knew, by virtue of his visits with project managers, that there was a roadblock of some kind, and he had probably informally offered suggestions for remedying the situation. More frequently, however, Watson prepared a critical analysis of the project team's problems and personally presented it to the assembled team. He usually went into some detail during these talks explaining to the designers how their design effort had fallen short of that which had previously been done within the organization. Review sessions of a similar nature were also given to all project teams in the group on a regularly scheduled basis.

Engineering Liaison

A final major problem that had concerned Gerald Ives ever since he joined the Atlas research organization, and particularly displeased him now, had to do with the outmoded procedures traditionally used within

the development section in designing new machinery. The antiquated method, which had been originated by the inventors in years past, involved the use of elaborate prototype models on which design refinements were based. This costly and time-consuming procedure was still in use in June, 1960.

As currently practiced, the design procedure was approximately as follows: (1) designers conceptualized their initial thoughts for a new design on a set of working drawings, from which (2) a test model was built. No calculation or tests were made before construction began. The designers instead relied exclusively on their "feel" to judge whether or not some mechanism would in actual fact, work. Usually, the older lead or head designers had developed their intuitive judgment to a high degree. The first model invariably functioned at least partially as predicted. Typically, this first prototype would (3) be sent to a nearby automobile plant for testing under actual production conditions and then (4) returned to Atlas where another set of drawings would be completed, correcting the flaws detected during the test. (5) A second model was (6) again tested but usually required a little refinement before (7) final manufacturing drawings could be completed.

As an engineer, Gerald Ives was familiar with and convinced of the utility of the use of theoretical calculations to predict design practicability, and he was extremely unhappy about this makeshift procedure. As one step in remedying the situation he had, soon after becoming laboratory director, increased the responsibilities and activities of the engineering group and eventually pulled the engineers out of the development section entirely. As the new engineering section began to prove its worth, a number of new young engineers were hired. Known throughout the division as Mr. Ives' "pet child," the engineering section was given the responsibility of updating the technical climate, improving research procedures, and providing technical consultation to the development section project teams.

Taking his cue from Ives' attitude, Greg Watson encouraged his designers to consult engineering representatives before the first model was built in order to explore the theoretical soundness of the proposed machine. Watson was also encouraging the project managers to help make designing procedures more efficient. During his frequent afternoon tours of the research facility, he would often stop and talk with a project manager, pointing out areas of the machine design where engineering help would be especially useful. In fact, most of the Group 10 project teams were employing some kind of engineering help. In most cases the young managers, whose recent academic engineering experience was still fresh in their minds, were quite prone to seek out engineering specialists on their own initiative to solve particularly knotty problems. Moreover, Greg Watson frequently called upon the engineering section manager to supply a whole team of engineers to make comprehensive tests or calculations on

machines being built. This was particularly true if project teams had made slow progress on a machine.

Watson hoped that the use of more engineering in the initial stages of the design sequence would eliminate the necessity of building more than one model and thus improve considerably the efficiency of the developmental effort.

Chapter 31 (Continued)

THE ATLAS CORPORATION (B)[1]*

GREG WATSON

GERALD IVES, laboratory director of the Atlas Corporation's research division, had made it quite clear to Greg Watson, manager of the development section, that the research organization as a whole looked upon performance on projects that the development section had under development as weighing the balance—either the research division would be respected throughout the company or severely criticized for failure to come up with significant results. Although Mr. Ives had planned for the research organization's long-term future by initiating several highly promising long-term research projects, the division's immediate future was dependent upon the satisfactory completion of machine design projects currently being worked on by the development organization. What the company needed most was some significant achievement that would restore their customers' confidence in Atlas' research ability. Moreover, the success of joint ventures with other companies into unexplored markets in most instances hinged upon Atlas' ability to design packaging, plastics-forming, or other specialized machinery which could be built and marketed quickly.

In addition to the pressures applied by Ives, Watson faced the possibility of a major reorganization which would affect the whole research division. Most of the company's other divisions had recently been analyzed by a team of management consultants hired by Atlas' president, and it was highly probable that the same procedure would be followed in the research organization. The end results of reorganization in other parts of the company had been the isolation of each section within a particular division as an autonomous organization with a hypothetical balance sheet and profit and loss statement, requiring the section to "sell" its services to the rest of the company. Mr. Watson hoped that his budget control system was adequate preparation for this eventuality. He believed it at least had the effect of impressing the need for economy and efficiency on project team personnel.

Another pressure on Watson and the development section involved the

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[1] All names disguised.

new engineering section. Ever since Mr. Ives had broken the engineering group off from the development section, Watson had watched the new engineering section grow. Although the work of the new section was regarded as overhead expense, since the engineers performed services for the other research sections, it too had some projects of its own. Most of these involved mechanical mechanisms of radically new design that offered outstanding potential as marketable items. Gerald Ives was known to be counting quite heavily on these projects as security for the company's future.

The activities of the engineering section were seen by Watson as dynamic and forward thinking. The section was staffed by a group of new engineering graduates—some with advanced degrees—and was by far the most envied job location in the research division. It was paradoxical that Group 10's two project managers had originally come from the engineering section, since one of Watson's most acute problems was keeping potentially good supervisory prospects from making the transfer in the other direction. It was a frustratingly common practice for the young acting project managers in the development section to request a transfer to engineering, enticed away by what they perceived to be the glamour and dynamics of the engineering section.

Greg Watson was, in fact, very much concerned about the ability of his organization to work effectively in the future. The inroads made by the engineering section into the areas of work normally handled by the development section, the transfer of top men out of the development section and the pressure for results from his superiors contributed to a feeling of considerable insecurity. Much of his concern centered around the older designers whose attitude, he believed, did not make his job easier.

In Watson's opinion the majority of the older designers had long since passed their most productive years and were simply coasting into retirement. Most disconcerting to him was their refusal to adapt to new methods for improving the efficiency of the designing sequence. All of them were, with only a few exceptions, graduate engineers and certainly capable of performing the necessary engineering calculations and test procedures necessitated by the new philosophy in design. Watson saw little reason for them to be interested in doing these things, however, since the company had always guaranteed their jobs and Atlas' present problems were quite probably of little concern to them. Watson's major worry in this area, however, was his belief that the older designers were influencing the younger men to continue the traditional makeshift design procedures which he was trying to get all of them to change.

Equally discouraging to Watson was what he called "the head and lead designers' lethargic work attitude." They appeared to be oblivious to the urgencies of the company's new competitive situation. Moreover, their thoroughness in "grinding out" every little detail to highest perfec-

tion was obviously blocking the way of design economies. The older design personnel seemed, to Watson, bent on making each machine a work of art—an achievement that was somewhat out of place when compared to the often reiterated need for developing machinery in a minimum amount of time. Watson did not advocate slipshod work. On the contrary, he desired a high degree of craftsmanship, but not at the price of wasted effort on marginal perfections.

Watson believed improvement of design quality could be implemented by requesting the engineers to test the designs conceptualized by the designers. He looked upon these technical liaisons as his "eyes and ears" in making sure nothing left the section which would subsequently fail under more rigorous and long-term production conditions. In contrast to the designers' prototype testing, Greg felt the engineers' techniques were more effective in detecting possible technical flaws in the machine design which might prove critical at a later date.

In his attempt to make design work easier, Watson felt that he had gone further than would normally be expected in liberalizing the company's work rules. Vacation periods had been extended and extra time off had been granted to many of the designers. He felt, however, that in many instances the older workers had overstepped their privileges and were taking advantage of his generosity. Several senior designers had bypassed lower echelons of management and had come directly to him with requests for special treatment. Though Watson granted their requests, he did so with reservations.

Watson felt his main hope for improvement of operations was to foster the development of the younger designers as acting project supervisors while at the same time retiring the older employees immediately after they had reached the mandatory retirement age. Cultivating the growth of young employees was not the only problem. Preventing them from leaving the organization entirely presented an equally challenging situation and one which Watson had not been able to handle to his own satisfaction. Both the engineering section and the sales division were constantly on the lookout for technically trained young men with management potential. These components of the organization were prepared to offer substantial increases in salary over those paid in the research organization, and within the past three years Watson had had to replace the manager of Group 10 twice because the previous supervisor had transferred to the sales division. In an effort to stop this trend, Watson had recently intervened in a transfer which would have sent Frank Richie, one of the two remaining project supervisors in Group 10, to the sales division. Besides meeting the salary increase which had been offered to Richie, Watson's only other bargaining point, as he saw it, had been to promise more supervisory responsibility and a chance for more rapid promotion.

Almost without exception a design engineer hired by the development section was given an immediate opportunity to perform the duties of an

acting project supervisor. Watson believed he sometimes appeared to encourage the candidates remaining in this position at the expense of demonstrating a real interest in the designers' role. He felt this behavior was justified, however, because, as he saw it, his primary need was to develop a supervisor for each project. Though many of the young designers had had their "time at bat" and, as Watson put it, "had struck out," others had received the nod and had been pushed rapidly into greater responsibility.

Of particular concern to Watson was the recent series of joint ventures with other firms. These "cooperative" arrangements required, he believed, project supervisors who were particularly adept at sizing up a new situation and then bargaining shrewdly. Watson believed he could help the younger supervisors to develop this attribute by offering himself as an example. He would, therefore, frequently accompany a project supervisor to conferences where representatives of other firms would be present and demonstrate the proper techniques of negotiation. A project management candidate who could not meet Watson's exacting standards in this area was almost automatically disqualified. He felt that individuals who could not represent Atlas' interests in negotiations with other companies had no business holding a supervisory position. In fact, one of the main reasons why Watson had put such a great deal of emphasis on developing younger designers into supervisors had to do with what he called "the singular talent for putting their foot in their mouth in most inopportune circumstances," displayed by the older men. Careless remarks, which at times betrayed a competitive disadvantage, might be tolerated if they were kept within the organization, but could hardly be excused, Watson felt, outside the company.

Watson appeared to be deeply interested in the training of the acting project supervisors. During his periodic afternoon walk through the section he habitually checked on the progress each had made. He expected supervisors to show "a healthy interest" in their work. He expected them to ask "relevant questions," and if these were not forthcoming and the supervisor had no personal problems he knew about, Watson made it a point to observe the man's work even more closely than usual. To test a candidate's alertness, Watson might make several suggestions for facilitating a project's progress and then, at a later date, would check back to see if the supervisor had made use of the information. He believed men with real potential for the supervisory job would "buy a philosophy"—a philosophy of approaching a problem in a certain way (using engineering assistance), cutting down the number of prototypes, and generally speeding up the whole design sequence. Although he found it necessary occasionally to be "harsh and uncompromising," he made an effort to follow up such episodes with a more understanding and sympathetic attitude.

While he felt the training as a whole was progressing rather smoothly,

Watson cited one frustrating exception—the young supervisors' reluctance to make decisions when they were obviously necessary. All too frequently Watson discovered instances where the failure to decide a course of action had cost the section days of wasteful effort. Usually, under these circumstances, he could see no other alternative than calling the group together and reviewing the mistakes, pointing out to the acting project supervisor in private, either before or after the meeting, what his specific omission had been. Watson did not relish these "Monday morning quarterbacking sessions" and frequently he would try to make them coincide with the periodic budgetary control reviews; but if this latter meeting was scheduled too far in the future, he believed it important to deal with the problems which presented themselves immediately. During these meetings he tried to develop a "fresh approach" by acting as a "catalyst" for new ideas developed by members of the project team involved.

Chapter 31 (Continued)

THE ATLAS CORPORATION (C)[1]*

Project Supervisors and Acting Project Supervisors in Group 10

THE PROJECT SUPERVISION system initiated in early 1958 was just one of several measures initiated by management and intended to improve the efficiency and competence of the research division's efforts. This case describes the attitudes and opinions of the personnel who had recently been appointed to the newly created position of project supervisor or acting project supervisor. (See the Atlas Corporation [A] case for amplifying details and organization chart.)

Most of the young designers and engineers who had been chosen by Greg Watson, the development section manager, to hold the project supervisor's position thought themselves fortunate in having an opportunity to participate in this work because the position was one of the few jobs in the research organization which offered an opportunity for a young researcher who ultimately desired a position in research management to demonstrate his supervisory talent. This was not to say, however, that all research personnel held such ambitions. In fact, most of the men attracted to the development section were interested exclusively in either a designing career or a technically oriented career of a similar nature. Only those individuals who, after being employed, changed their career goals were interested in project supervision.

For this minority group, however, the opportunity to become a supervisor was a vital incentive because the designer's position offered little else to men who were not completely absorbed in the work technically. Once a man achieved lead designer status, which could be easily accomplished in 20 years, there were no higher positions available. Individuals might be assigned to more complex machine design projects, but these were limited in number. Salary increases were periodically given, however, right up to a designer's retirement, and the higher scales paralleled those of section managers. But to individuals eyeing positions in management, the "stepping stone" opportunities offered by the project supervisor's position were far more attractive than the designer's predictable increases in salary.

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[1] All names disguised.

In contrast to the designers' close-knit group, the acting and full-status project supervisors seldom met together as an informal group. Most of them were actively engaged in project work and, assuming relations were not strained for some reason, they usually chose their friends from the younger members of their project team.

It was with the older designers that the project supervisors were experiencing their greatest trouble. Many of the supervisors believed the head and lead designers were preventing them from establishing themselves as an integral part of the project teams.

The following series of excerpts from interviews with project supervisors and acting project supervisors in Group 10 serve to document these and other sentiments.

Frank Richie—Project Supervisor

Frank Richie, 33, had joined the research division some eight years previously after completing his engineering degree. He had originally worked in the engineering section where he had been a test specialist on various new mechanical mechanisms that were presently being integrated into machinery being developed by Group 10. After completing the "basic" design work in engineering, Richie had transferred to Group 10 in order to facilitate the development process by supervising the designing of several machines which had practical applications of his ideas built into them.

Although Richie was devoting the majority of his time to one particularly large and crucial project involving the design of a radically new ice cream carton forming machine, his duties also included supervising several other projects which had equally high priorities. The "carton-making machine" (referred to as the "cream machine") was the largest project assigned to Group 10. The project team was staffed by six designers and four "borrowed" engineers from the engineering section. The project had been started over five years before but had been recently accelerated when a national dairy showed enthusiastic interest in the machine. Richie and the project team were working against an extremely tight deadline and high performance expectations. The development of the machine was a joint venture with another company which was developing the material for the ice cream cartons. Frank Richie expressed the following views about his role to the case writer:

When I first came with Atlas I thought I wanted to be an engineer and nothing else, but it sure wasn't long before I discovered that strictly a technical orientation just wouldn't lead anywhere—as far as the whole company was concerned. In fact, after 20 years there is just no place left to go. That's why I chose the project supervisor's job. I think you have more opportunities for advancement. But it's a lot of pressure and headaches, and I'm not at all sure it's what I want.

The older designers resent the fact that I'm younger, especially Paul [Paul Clayton, cream machine lead designer]. Just getting their cooperation is one of my chief troubles. Actually I have a lot of responsibility and no authority. What bothers me most of all is that I've got all these projects to supervise and particularly the cream machine, and yet I can't get these people [designers] to work for me. Do you know every time I want one of my decisions to stick, I have to run to Greg [Watson] or Andy [White] and have them back me up.

Maybe it makes them [designers] mad but I feel I should get right with them and get my hands dirty while building this machine. I guess I could direct a whole project by just standing back and letting them do all the dirty work, but I feel I'm a better leader when I know what they are doing and contributing my technical skills. After all, I've worked with this type of thing for some time now and I have a pretty good idea what should be done. What's getting on my nerves now is the haphazard way we are approaching the whole program. I just can't persuade these designers to go along with me on some systematic and logical test procedures—using engineering tests to check things out before we go ahead and build. They keep insisting that you build first and then alter if something is wrong. We're getting down to the wire and I'm getting shaky—wondering if I shouldn't have stuck to my guns and brought in some more engineers anyway. In some cases I've done that. And it was sure a good thing, because if I had let them go, we wouldn't be this far along. But the designers aren't the only ones—I have trouble selling Greg Watson on stopping where we are and mapping out a test program. Sure, we are in a crash program and I know the idea is to get the machine completed, but it seems to me in the long run we would be better off by taking a little more time now and proceeding logically. I have a feeling that everything is going to fall in on us at the last moment, if we don't.. Greg's attitude is just to get the job done, which I agree with, but I feel it's my responsibility to see the machine works when we are all through.

We are moving at such a frenzied pace that no one takes the time to keep any records. I've wasted two days this week checking out a design I know we did before, but I can't find any records of the previous results. The designers never have put their work down in writing, and they are not about to do any recording now when we are in such a rush. If they would take a little less of their time on perfecting every little detail of the design and put a little more effort into writing up their findings, we would be much better off. I guess this emphasis on obtaining patents for every new mechanism is a carry-over from the old days, but I personally feel it's a crutch they're leaning on. Patents are worthless today; the idea has got to stand the test of the market. We waste so much valuable time just perfecting these little patent details; we don't have time to put effort where it really counts—on the perfection of the overall machine.

No one seems to have the overall picture; everybody is busy with their own little part, and we lose sight of the "big picture." Frankly, I'm up against a stone wall—no one will listen to me and the project is suffering because of it.

Kenney Tubbs—Acting Project Supervisor and Designer

Kenney Tubbs had been with Atlas for ten years. The first five years had been spent in the firm's sales department, where he had been a liaison representative between the latter organization and the development section. In 1955, he was employed in Group 10 as a designer and began

apprenticing under some of the older head and lead designers. Shortly after Gerald Ives had initiated the project supervising system, Kenney began handling some of the "economic" responsibilities associated with the acting project supervisor's position. Before long his assignments included three active machine design projects, on one of which he was both the designer and the acting project supervisor. The other two machine projects took less of his time since older designers were handling the majority of the technical work, while Kenney handled the budgets and report-writing details. All three of these projects were considered "rush" items, and Greg Watson was known to be quite anxious to complete them in a minimum amount of time. The following are excerpts from the comments Kenney made concerning his work in Group 10.

My head's in a swim; I have got three projects going on now and I don't know where I am. All of them are rush projects, and yet I don't know which one to devote most of my time to. I asked Andy White what to do, and he said put 100 percent effort on the one I'm designing, and yet I just get to work and someone comes around and asks a three-hour question. Andy and Greg just don't seem to realize how much time it takes to design. They seem to think you just put down a couple of lines—finagle them around—and you're all through. Well, it's not that easy.

I feel—and this isn't my opinion alone—you'll find it prevalent through the group—too much of our work is being directed from the boys upstairs. All the thinking is done for us and what's worse—we are told how to think and even how to write a report. For example, you write a progress report on a project and put your John Doe on it. Andy doesn't like the way it sounds, so you have to change it a little bit—whether or not you are in complete agreement. What I want to know—why don't we write a report just exactly the way we see it and then let the boss write a letter to go along with it. Sure, a fellow could hang himself, but he might profit by the experience. Where do you draw the line between being cooperative and having a mind of your own? It seems to me there are too many fingers in the pie. Everybody is trying to get in the act. Responsibility flows around here too loosely. I would like to see it where you could either sink or swim. Where you could take the job all by yourself so that you could nail somebody down as being responsible for something. I don't know where somebody's responsibility ends and somebody else's takes up. So many people are mixed up with one decision or problem that you don't know who to single out for errors.

This present setup of being a pseudo-project supervisor where you have no authority is for the birds. Under the present system, if a younger acting project supervisor tells an old prima donna designer to do something—he is told to go fly a kite. Now if you were given full recognition by management as "the" project supervisor, then everything we said would carry more weight and be backed 100 percent by management. I think one of the biggest stumbling blocks in the whole system is that the old designers aren't used to regimentation. I know—if you don't win a person's cooperation in the long run he'll undo you by not putting his whole self into the job—but that's human nature. Sure—in a good project supervising system there would be a lot more fighting and hair pulling, but things would move faster. Right now things are going too slow.

I guess what's bothering me is that I really don't know where I stand. No one has told me what my classification is. Off the job people ask me what I'm doing and I don't even know what to tell them. Greg Watson tells me that I'm an

acting project supervisor, and Andy White tells me I am a designer. It looks like I'm playing all positions. If you don't know where you're going, then I think you're in pretty bad shape. I'm left to drift. Maybe there's a motive for all this but I can't see it. I've heard, "words" from Watson and White but it wasn't an explanation—just a pacifying action. It's like giving a crying baby a candy sucker so that he will forget about what's on his mind. What's more—I don't know what I'm supposed to do to get ahead in this organization. I just don't understand what they want. Greg insults my intelligence when he comes around here asking me if I looked into this little matter or that. I got a good chewing out the other day because I let some drawings go through with some bad tolerances on them. If all I did was worry about tolerances—that's all I would get done. We'll be going around and around on this for a long time. You wait—when it comes time for a raise they'll be bringing up the whole thing again. Greg and Andy come around and bitch at me when I'm doing something wrong, but when I'm doing a good job nobody comes around and pats me on the back.

Bill Goodwin—Designer and Erstwhile Acting Project Supervisor

After graduating from engineering school in 1957, Bill Goodwin had joined Development Group 10 as a designer working on automotive machinery designs. After an initial assignment as a junior designer, he had been transferred to another project team where at the request of Greg Watson he began handling the acting project supervisor's responsibility. Within the last year he had been transferred again to another project where he resumed full-time designing responsibility. Following are Goodwin's comments concerning his brief stint as acting project supervisor.

When I first came here I just didn't feel I was using all I had learned in engineering school. I was filling in my spare time with detailing[2] in order to keep busy. I just didn't feel I was getting anywhere. I thought if I got on a larger project there would be more engineering problems. When you're dealing with automobile machinery, you're just so far behind the older designers that you wonder if it's possible to make any contributions at all. They've got their own way of designing.

When I transferred I started to handle the project supervising duties. I guess I aspired a little higher than what I was capable of. Greg told me I was the acting project supervisor, but actually I was doing nothing but the paper work. He told me after I was on the job for several months that I wasn't exerting enough initiative in getting the designers moving faster. But it's ridiculous to think I could direct the efforts of a designer who had 17 years' experience. I tried to convey this to Greg, but I don't think he understood. It's a waste of time right now to try to keep tabs on a project's progress, no matter who keeps the records. The designers are going to move at their own pace, no matter how much time and money management allocates.

The old designers are getting used to the system. I think they have been very tolerant, but we need a clear delineation of responsibility. Let the designers do strictly the designing and nothing more—don't burden them with any of the

[2] A design task usually assigned to beginning designers which involved making detailed drawings of individual parts. Quite frequently thought of as a menial responsibility, it in fact required a great deal of skill and judgment which research management thought a prerequisite before attempting more advanced work.

administrative details. If the project supervisor is going to be held responsible for the team's progress, then he's got to "manage" the team, not just do the paper work. As it stands, it's all mixed up on who's doing what.

Victor Baum—Project Supervisor

Victor Baum had been with Atlas nine years, the last two of which had been spent with Group 10. His first seven years with the company had been spent in the engineering section. Baum's views were as follows:

I'm another fugitive from the engineering department but I'm not sure I've found my happy home yet. This supervisory work is new to me and I still let my designers make all the technical decisions.

I think the system [project supervisor] is okay. It has freed up the designers so they can devote full time to designing. Actually, I rely on their judgment quite heavily. But there are a lot of "economic" problems that have come up in the last few years which a project supervisor has to worry about exclusively. Just keeping the budget straight and working out the design specifications takes a lot of my time.

The thing I'm not so sure about is where I go from here. The higher you go up in the ladder, the more of a politician you have to be. The way I look at it, management's job is to see that the work gets out and keep the people happy. I guess I'm an engineer at heart, but still project supervision looks appealing.

One thing that's been causing a lot of trouble with some of the other men [acting project supervisors] is that they [management] didn't do a very good job of explaining the system to the designers. When I first came in here no one knew about it. When I was made an acting supervisor they gave me the manual to read. If the designers ever saw it [the manual], it would really shake them up. Just the way it is written would give them the wrong impression. What they should try to do is get across to the designers that we're not taking anything away from them, but just giving them more freedom.

Pete Steiger—Acting Project Supervisor

Pete Steiger, 24, had come to Group 10 three years ago after graduating from a nearby engineering school. He was still working on the same project that he was assigned when he started with Atlas, but had recently taken over the duties normally handled by the acting project supervisors. There had been no announcement of this change. Pete's views were as follows:

Everybody is friendly around here and I'm enjoying my work. I'm really a Jack-of-all-trades, on the drawing board part of the time and then I break this up with a little bit of clerical work, writing specifications and keeping budgets. I like the pattern but I don't know if it will last forever.

Greg Watson has given me three years to decide whether I want to become a designer or a project supervisor. He thinks I should show some inclination one way or another in that amount of time, but what this inclination is I'm not sure. Right now I don't know what I'm going to do exactly. I really like the creative part of the design work, but I want to be sure if I can eventually become a good designer before I can commit myself one way or the other. I get a great deal of

satisfaction out of creating a good design, but I just want to be sure I can earn my salt doing it. Right now I guess I'm leaning towards management—that's the way they want me to head. You know—if they throw you a ball that way you had better catch it, although I sure wouldn't want to go exclusively into management if I couldn't sneak in a little design work at the same time.

The project supervisors have got higher status around here. When Mr. Ives and Greg Watson come down here for a visit, they'll always stop and talk to Frank Richie but pass right by the older designers as if they hadn't seen them before. You know what my first boss told me? He said designers are a dime a dozen and the real money is moving up to higher supervisory positions. I'm still not convinced though. It looks to me that straight supervising is too straightforward, routine, and no challenge to it. Besides you have to be too much of a politician.

I don't like the management attitude of settling for something less than best. I guess the engineer in me just won't let me turn out something that I know is not right or I'm not sure of. For example the engineers and I were working on a problem that was really giving us a rough time. We had spent about a week running various tests when Greg Watson came by and said not to worry about it any more and let it stand the way it was. He even seemed disturbed that we had spent too much time already.

I guess he had a good reason for making the decision. At least I'm covered. But one of the engineers told me that if something goes wrong after a machine is out in the factory it's a good way to lose a job. I understand Mr. Ives is a strong technical man who won't tolerate sloppiness. I'm in no position to carry the ball any farther. Who am I to dispute management decisions?

Greg has always been pushing to get this project out. It seems we never work fast enough and are always behind schedule. Last April we were told to revise the schedule to see if we could get the job done faster, but I looked over it and it was tight. Then Greg came down with his statistical charts and said we had the worst record in the company, which didn't make us feel any too good. There were some charts and graphs on how much time each drawing should take, figured on the basis of the averages of all the other projects in the department. It seemed logical and I guess you have to have something to go by. None of us had too much to say. He told us just to get the machine out and not to worry about obtaining the ultimate.

Chapter 31 (Continued)

THE ATLAS CORPORATION (D)[1]*

The Designers in Group 10

Many of the organizational changes research management had initiated in order to improve the efficiency of the development effort were having a marked effect on the designers in Group 10. The older personnel, especially, were experiencing a difficult time adjusting to the new methods and organization. This case describes the attitudes and opinions of several of these designers. (See the Atlas Corporation [A] case for amplifying details and organizational chart.)

Under the new procedures, the role of the designer within the developmental sequence was gradually changing. Although in most instances he continued to perform technical design work, many of the administrative functions he had acquired over the years had now become the responsibility of the acting project supervisor. Even within the design area, the designers were being encouraged to make use of engineers to assist in the research effort. The adjustment was not being made easily by designers whose long-ingrained work habits and methods were completely different from those management was proposing. Moreover, there appeared to be general confusion on the part of the designers concerning what management expected of them.

The designers in Group 10 had always prided themselves on maintaining a warm and cohesive work group which, in the eyes of many Group 10 people, contrasted sharply with the rest of the research division's subordinate organizations. They derived a great deal of pleasure from working together, and considered themselves a highly productive group. For many reasons, this situation appeared to be gradually changing since the adoption of the project supervisor system and the implementation of budgetary control procedures. Several designers suggested the possibility that the group was not ready for the project supervisor system, since the individuals assigned to the supervisor's role had not sufficient background to effectively direct older, more experienced researchers. Also, they believed the limits of their responsibility and action had been considerably reduced from those they had previously experienced.

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[1] All names disguised.

The following series of excerpts from interviews with designers in Group 10 serve to document these and other sentiments.

Stan Lomax—Lead Designer

Stan Lomax had been with Atlas for over 20 years and was considered to be one of the most experienced and knowledgeable designers in Group 10. Before coming to Atlas, he had spent over 20 years in machine design with other companies in a variety of industries. The following are excerpts from Lomax's comments about his experience with Group 10.

The organization of the work down here is not like the other companies I've been with—none of my bosses can tell me if the job I'm doing is right or wrong. They're not technically competent to direct me. Why, I trained Andy White as a designer and now he's my boss. You can't say he's better than I. As far as I'm concerned the group manager is just a clerk doing a little bit of routine work—how can he direct me? This place is not very well managed!

For lack of supervision they try to make up for it by giving all sorts of written specifications so we don't drift off the beaten track. But they don't mean very much—they don't tell me what I'm supposed to do. Take for example, if we knew something about the market and the chances for success for the machine we were building, we would know whether to skimp or spend a lot of time in building a good one or not.

Another thing that really gets me is the way that they think we can work miracles down here. Without even knowing what we are doing, they think we can design a machine perfectly on the first crack. But how can you be sure of what you are doing without building at least two models? Management just doesn't understand! I wish Greg [Watson] would come around when we have the machine on the drawing boards rather than waiting until we've got the first model built and then bitching about how slow we are coming.

The project supervisor system isn't really too bad, but we're not ready for it—the project supervisors aren't experienced and I can't see any need for it now. I'm supposed to be working for some sort of an acting project supervisor [Kenney Tubbs] but I don't see him very often. About the only time he comes around is once a month when he has to write up my project progress report. He doesn't know what I'm doing, and usually the report isn't anywhere near the existing situation. It looks to me as if they're putting in one more level between the designer and management. Putting the designer further out of the picture! These young men just don't have the experience the designers have, and consequently, they can't direct the project. All they really can do is handle the clerical details. Now if they would appoint some of us older people—in fact I wouldn't mind being a project supervisor; of course, I would have to change my way of thinking and my habits, but I've done that before. The supervisor—he thinks differently—he's a suggester, thinks of how many ways you can do the job while the designer has to sort out the ideas. You know, I think management is trying to compensate for the way that things were when the inventors were around here—only they're overcompensating!

Phil Singer—Designer

Phil Singer had been working in Group 10 since the end of World War II. Although his education did not include an engineering degree, he

had had considerable experience within the organization on some extremely difficult design assignments. He did not presently have the title of lead designer. However, it was most probable that with several more years' experience he could easily qualify for that position. He was presently working on the ice cream carton project under Paul Clayton. His comments were as follows:

Well, my general feelings are that I've had a lot of experience and knowledge to add to the project but nobody asked me for it. I've had plenty of background in packaging machinery, and I thought on this ice cream project I could really help out, but I don't get invited to any of the meetings or asked what my opinion is—so I just do what I am told. Frank Richie [project supervisor] doesn't ask me, so why should I say anything? My beef, and I think I'm speaking for the rest of the designers, is that we're not consulted enough on how we would do the designing. This push, push to get things out! We can't do the engineering and testing until we have a machine to work with. We have to have a model first.

It's just a general feeling I have—15 years ago we were one big happy family where everybody knew what was going on. You felt like you were in on everything. Right now I don't know what's happening upstairs in the other departments. You can't wander around any more, but I guess we have more responsibility and that makes for more pressure. The jobs are divided up more so that it keeps you busy. I consider myself fortunate, though, that I am able to design so much. When the inventors died out and the designers took over, they gave the biggest share of the project responsibility to the designers.

This project supervising system is not working out too well. Before I came to the project I worked with Kenney Tubbs [an acting project supervisor] and he had just too many projects to look over. Well at least I didn't see too much of him. He would come around once a month to find out what we were doing so he could tell management in a report. Now you know it's pretty hard to get anything straight, especially when you're going through someone who doesn't really know what you're doing. It would be different if he would sit down with you and talk the whole story over, then relay information higher up—that would be a lot better. Or maybe if he could spend two or three weeks with you.

Most of the information on the project [supervising] system comes through the grapevine. I really can't remember when this whole thing got started. But as I see it, the project supervisor is the coordinator, the report writer, the one who keeps management up to date on progress; he certainly doesn't have the overall control. I can imagine what would happen if they tried to give the project supervisor the whole thing! Boy, the old designers would really be up in arms. What we have now is too much direction with a lead designer and a project supervisor both.

I don't like the unequal opportunities the project supervisors receive. They get to go out and go to all the trade shows and are able to keep up with all the latest developments in the industry, while the man on design is supposed to stay here. I haven't even been outside to the [automobile] factory for two or three years now. How can I keep up with the industry if I don't get out? No wonder a lot of people won't stoop to ask a designer.

George Woodfield—Lead Designer

George Woodfield, 62, had been with Atlas for over 35 years, the majority of which had been spent in Group 10. He was known for his

wide range of designing skills, particularly on automotive machinery. He was presently involved in redesigning an extremely complex machine that Greg Watson was anxious to complete. Woodfield's comments were as follows:

I really like the work down here and especially the people I work with. They're a grand bunch, but I don't think management has got the proper appreciation of the difficulty we are experiencing in redesigning this machine. They thought the job was nine tenths finished when they gave it to us. According to their thinking it was as simple as making some minor changes in the drawing and letting it go at that. When we got the machine it was practically held together with baling wire. We had to redesign over half the mechanisms. Management keeps putting the pressure on us so we will get the job out. If they really wanted me to move on this thing they would have let me pick my own team instead of giving me two green designers.

It's a funny thing; when a man reaches a certain age around here they think he is no longer useful. Look at old Oscar Brandt. He's one of the best designers this group has ever seen, and yet Greg is retiring him from the group. Oscar's got ten more productive years. They let him go and take in a couple of these young punks who can't possibly be useful for a couple or three years. Oscar's being hired back by another department of the company as a checker! Can you imagine Oscar checking drawings of people that don't even know one fourth as much as he does? At least they could keep him on and let the young ones work underneath him. He could teach them the tricks of the trade.

And how about Pete Steiger? He's only been around here for three years and already he's an acting project supervisor. Andy White is really the project supervisor, but we don't see much of him. They are kind of pushing it on him, but his heart is really in the design end of the business. He goes to all the meetings we have about the project. You would think they would let me go some times. I'm the lead designer on the project. I guess they think I don't have much to offer.

Oscar Brandt—Lead Designer

Oscar Brandt, a graduate of a well-known eastern engineering school, had been with the company for over 20 years. At the time of the case writing, he was preparing to retire from the research department. As mentioned by George Woodfield, he was being re-employed by another department as a part-time employee checking engineering drawings. Before coming to Atlas, Brandt had had a diversified career as an engineering designer with other companies. His expertise in many fields of automobile machinery design was frequently called upon when other designers faced difficult problems. While for the most part his past assignments had been as a lead designer, he had spent the last year helping George Woodfield on the latter's "rush" project. The following are excerpts from comments Brandt made just before his retirement from the research group:

It has been enjoyable working here. The people here are swell to work with. I sure wish I could stay, but there doesn't seem to be any room. Although I guess I didn't like the work real well except for the inventiveness that it

required. I like to use my creativity and originality. But lately there hasn't been much use for that.

I've worked for weeks at a time without anybody bothering me. I think I would have liked to have someone come around and at least see what I was doing. Greg [Watson] and Andy [White] should come around more often and be aware of our problems. We've had to design some ridiculous stuff that wasn't even practical because they didn't appreciate what we were doing. And that was after we had tried to tell them. Andy White is a good administrator and I like him as a person, but he should come around here more often and take an interest in what we are doing. Maybe he is talking to Woodfield, but I never see him. Management seems to be cooperative and friendly but doesn't have the time to see what we have to offer. On the punch press machine project I made up a simple sketch of a design mechanism that a cost engineer estimated would save some real money to the company, and yet they [management] didn't want to even take a chance because it would take too long to field-test it, and yet there were other parts on the machine that weren't even field-tested either. I don't think they make decisions with the proper appreciation of what the implications are. We don't see the reasoning behind their decisions.

Paul Clayton—Lead Designer

Paul Clayton had been with Atlas for 40 years, all of which had been spent in Group 10 as a designer. Although the group had no official ranking, it was generally agreed that Paul was the top designer within the organization. Moreover, Paul had consistently been assigned the most difficult projects worked on by the group, evidencing management's belief in his superior design ability. His present assignment was as the lead designer on a new ice cream carton forming machine. It was considered the group's and the section's "hottest" project. Paul had started the project some 18 months previously and was currently "debugging" the machine for adoption within the next five months. Ten months earlier, Frank Richie, the newly appointed project supervisor, had been assigned to the project. The following are excerpts of comments by Paul.

I've grown up under the old system around here. Just like the inventors, you were assigned a problem and you followed it through to completion. That's the way I work. You have an idea on how you are going to build something, you make a model first of it, so you can test it out. That's the only way to know if it works or not. If the model isn't right, throw it away and start over. It's the idea of getting something out in three-dimensional perspective so you can take a look at it, fiddle with it. You can't see if it's going to work while it's still on the drawing board; a 3 D model lets you improve on what you've done.

The lead designer really has got the big job of overseeing the whole machine design—seeing that all the work, all the other designers are doing their job and tying all the loose ends together. The project supervisor keeps all the paper work and writes up the progress reports on the job. The project supervisor has got to get some background on what we designers are doing before they can be of any help. They're too biased when they first come and don't have any appreciation of all our problems. They should be asking, where can I help, instead of wanting to sit behind a big desk immediately. They have to start down at the bottom—get the problem from the beginning and not tell the other

persons what to do right off the bat. A lot of young fellows come in here and don't appreciate the fact that you've got to start at the beginning. The project supervisor has no mechanical foresight. How can he tell the designer where to go? The project supervisor is at the mercy of the more experienced person.

Management sits up there and tells us everything should have been done yesterday. They expect you to solve everything overnight. But if they would come down here and see what's going on it would change their picture. When you're on the job the picture is a lot different. Management wants that extra 30 percent over and above what we're doing, but they're not just technically aware of what's going on. I'm working just as hard as I can—even carry a little notebook when I'm at home. At night I write up ideas that come to me and look them over the next day.

Chapter 31 (*Concluded*)

THE ATLAS CORPORATION (E)[1*]

The Ice Cream Carton Machine Project

THE MACHINE DESIGN project involving the development of a radically new method of making ice cream cartons was the most important project being worked upon by the development section of the Atlas Corporation in the summer of 1960. The corporation was presently involved in a struggle to regain a dwindling market position, and the project was looked upon as a vital element in the company's new diversification program that, in turn, was expected to lead to higher sales and profits. (See Atlas Corporation [A] and [B].)

The ice cream carton forming machine (referred to as the "cream machine") represented Atlas' first major attempt to design a machine for a market other than its traditional sales area—the automotive industry. Immediately after it became apparent, in 1955, that it would be impossible to rely exclusively on this latter market for a high sales volume, work had commenced in the research division to develop the mechanical principles necessary for rapid production of a large (5–10 gallon) ice cream carton. The cartons were to be made of a new synthetic combination of fiber and plastic. The new material, developed by the Maroon Chemical Company, which was cooperating with Atlas on the project, was to be formed into cartons on the machine at a rate far superior to that of machines presently operating in the industry. Moreover, the cartons could be sold at nearly half the price and were less than a third of the weight of traditional bulk ice cream cartons.

Gerald Ives, laboratory director, and Greg Watson, development section manager, were particularly interested in this project because many of the principles involved in the machine design, once proven in practice, appeared to offer excellent possibilities of exploitation on machines destined for other industries. In fact, both Ives and Watson were relying quite heavily on success in this venture to open up new opportunities for diversification. A considerable portion of the R & D budget had been spent on the cream machine project and its component parts in the hope that the "payoff" would not be limited to one industry. In addition, the

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[1] All names disguised.

machine's development was being financed jointly by Atlas and the well-known chemical company which had developed the synthetic material used in the carton. Atlas' research management was extremely eager to make a good impression on this latter firm so that a continuing relationship might be developed. Accordingly, it was imperative that the machine meet the highest technical specifications and be finished by the scheduled completion date.

The present cream machine was a modification of an earlier machine design for what subsequently became an abortive venture into the cheese industry. Although the machine had functioned satisfactorily at the laboratory stage of development, adverse production conditions at the cheese factory had caused the model to fail periodically. The contract had been revoked when the machine could not be made to function continuously (24 hours per day) within the exacting technical requirements.

The present revision had begun in early 1959 when a national dairy had shown an interest in both the machine and the new carton's superior quality. The dairy had contracted for one experimental model to be delivered in November, 1960. The exceedingly short development period was necessitated by the dairy's insistence that the cream machine should be operating at the same time production began at the new centralized ice cream plant where the machine was to be installed.

The original project had been under the direction of Paul Clayton, one of Group 10's oldest and most experienced lead designers, but when the latest contract was awarded, Greg Watson had assigned Frank Richie to the team as project supervisor. Watson acknowledged the fact that the project was in reality Paul Clayton's, if for no other reason than that Paul had supervised the total design of the machine up to the time of the redesign and had completed this work in an inordinately short period of time. But as Watson stated it, "Paul was like a bull in a china cabinet and uncommonly stubborn," which in Watson's opinion was hardly the most desirable behavior and attitude for a leader of what was now such an important and crucial project. Watson said, "Frank Richie really has to be a stinker in the team, trying to shake out some of the kinks." Watson had decided to assign Richie to the team because of his faith in his potentialties as an effective leader. "Why, eventually he will pass up Andy and maybe I'll be working for him some day, because he has picked up the techniques of working with people." Watson was deeply concerned with maintaining Richie's interest in project supervision, and giving him such an important assignment was his way of enhancing the possibility that he would stay with Group 10.

Watson told the case writer: "Keeping Richie in this section is one of my biggest problems. The sales division once dangled a big salary offer in front of him. I was about to lose him when Gerry Ives told me to meet the offer. I was a little surprised at the suggestion, but that's what I did, even though his salary is way out of line with the rest of the boys' now. You

might say I bought him. We played with his life a little if you want to call it that—all above board of course. I think he's still a little undecided about this whole project supervising business. If Frank had his way, he would probably like a laboratory off in the corner, where no one would bother him. But he'll grow into it."

Another reason Watson gave for assigning Richie to the cream machine project, beyond that of providing the team with an effective leader, was Richie's extensive experience with the design of many of the machine's component mechanisms. In fact his entire tenure with Atlas had been spent in working with the most crucial part of the machine—initially in the Engineering department, where he had worked out the basic principles, and more recently, before taking over the project in Group 10, where he had designed the practical application.

During the summer of 1960, the basic design of the machine had been completed, and in fact several hundred test cartons had been run off. The machine was, however, far from completion. There remained many technical problems, which in some instances surpassed in difficulty the work which had already been completed. Several of these barriers were complex problems requiring the solution of one portion before another could be tackled and closely resembling the type of problems where a "system" approach had been found effective in the past.

In order to meet the tight delivery date, Greg Watson had tried to enlarge Paul Clayton's original team of three designers to a much larger and more experienced group. Two of the team's original designers had been transferred by Watson because of "their questionable attitude and lethargic work methods." In their place, two other lead designers of known competence were assigned to the team along with a junior designer who had previously demonstrated an ability to work well and rapidly. Frank Richie had suggested that several engineers be temporarily assigned to the project for technical assistance on particularly difficult and complex problems that the designers were incapable of handling. Watson had complied with this request and in addition obtained the full-time services of a packaging engineer who had previously worked in the plastics field. By employing the latter, Watson hoped to provide the team with technical packaging experience not previously available to them.

From the beginning the team had been plagued by a series of interpersonal conflicts which had noticeably affected the project's progress. In many instances Watson or Andy White had felt compelled to mediate and restore order before arguments between various team members threatened to halt work completely. Conflict appeared to center on the relationship between Frank Richie, the team's project supervisor, and Paul Clayton, the lead designer. Frank Richie's problems were not limited, however, to establishing effective rapport with the latter individual. The other designers on the team, especially the older ones, quite frequently disregarded Richie's directions.

As early as 1958, when the assignment was first made, Paul Clayton had

opposed Frank Richie's appointment as project supervisor. At that time the project supervisor's actual duties were relatively unknown, and in fact Frank Richie's appointment was the first to be made in the section. When Clayton had initially expressed unhappiness about the addition of Richie to the team, he had been told that Richie's main function on the team would be to keep the various records organized and to handle administrative details. Since that time, however, Richie's responsibilities had considerably expanded, and the encroachment on Clayton's role as the project director had precipitated many conflicts between the two. More recently, although there had been no general announcement to this effect, Richie had been treated by Watson as though he were the team's supervisor, responsible for both technical and "economic" matters. Andy White, manager of Group 10, thought one of the reasons this had happened was that Richie had gained the team members' confidence by "remaining cool and collected while Paul gets panicky and starts pulling knobs and buttons every time something goes wrong with the machine."

There were a great many matters on which Richie and Clayton did not agree. Each of them appeared to be taking basically different approaches to design procedure. (Review comments made by Frank Richie in Atlas Corporation[C] and Paul Clayton in Atlas Corporation [D].) Richie, attempting to promote logical test procedures and systematic records of test results, was encountering resistance from Clayton, who stood fast on his belief that the traditional method—building the mechanisms first and then redesigning—was the only way to make progress.

Several recent incidents served to illustrate the kinds of problems the team was facing.

Maroon Chemical Company had recently sent their chief engineer, Tom Vycal, to visit Atlas in order to check design progress on the cream machine. The meeting between the project team and Vycal had turned into an inquisition, the latter asking highly technical questions on the progress of the cream machine to date, which, in many instances, the project team could not effectively answer. Frank Richie made the following comments to the case writer on his reaction to the meeting: "We were really in the hot box with Vycal inquiring into everything. He kept suggesting why didn't we design this way or another or try another method. Paul kept saying we had tried each of the suggestions before and they didn't work. But Paul didn't have any proof and we looked kind of bad. I luckily remembered that the engineers had made some tests on what was causing the differences in the size on the cartons and finally got them down here to explain what they had found. Sid Ball [an engineer] came in and laid out a beautiful set of data made up into charts and graphs which immediately shut Vycal up. It sure proved to me that we have to keep records on everything we do. The whole design procedure has just got to be formalized so that we know where we're going. Vycal is coming back and next time I really want to be ready for him."

Shortly afterwards Paul Clayton told the case writer quite a different

story: "I don't know why they've got to go and call in the engineers with all their graphs and charts—I already told them why we were getting different size cartons. I drew a little diagram and showed them why—but they wouldn't listen to me. They wanted better proof. I got mad. I bet my pay check that I was right. I don't have time to keep records. I wouldn't get anything done if I recorded everything I did; besides, that's what Frank Richie is for. When he came in, he was supposed to keep all the records, but he didn't do it. The old inventors didn't have time to keep records. When they tried something out and it didn't work to their satisfaction—they discarded it and tried something else. When I build something and try it out, if it doesn't work, I put it away. You don't have to keep records on something that doesn't work. I can't work like Sid Ball—he's too slow. I would get all agitated if I had to constantly keep records on what I was doing all the time."

Following the meeting with Vycal, Frank Richie's days were spent in arranging for more engineering tests to be performed on problems facing the group. In addition, much of his time was spent in actually overseeing the activities of the designers. Paul Clayton was also concerned about the activities of the other team members, and the two would often disagree violently on the priority of various phases of the design. Moreover, there was considerable disagreement on just how much of the previous design should be sacrificed to make the redesigned machine more functional. Frank Richie complained, "Paul's a firm believer in using parts and designs that are already made, but that's foolish. I'll build a whole new machine before I accept any second-rate design." Richie was also having a great deal of trouble assigning specific work to Paul. The elder designer preferred to work on problems which interested him but which usually did not coincide with those Richie thought vital. Moreover, Clayton quite often chose to concentrate on small projects that involved actual work on the machine. This often conflicted with engineers making tests for Richie.

The following conversation between Richie and Clayton, which took place in Andy White's office, was typical of the disagreement that often resulted when the two men discussed the cream machine project. Present during the discussion besides Richie and Clayton were the case writer and Andy White.

PAUL CLAYTON: [*Coming into the office and interrupting a conversation between Andy White and Frank Richie.*] Do you know the engineers are going to have the machine tied up for the next three weeks! I can't get in there and do my work with all them buzzing around.

FRANK RICHIE: What can we do? We've got to have the tests and they are going to take some time. What's the problem?

PAUL CLAYTON: I've been trying to figure out a new way of loading the raw material onto the machine, and I'm not going to get any place if I

can't operate the machine by myself. I still think using the cylinder the stuff is shipped on will be all right. What we have is good enough. Why do we have to change anything anyway? I don't think you can find a better way.

FRANK RICHIE: We'll find a way. Don't you worry. This thing has got to be foolproof. The way it is now some inexperienced operator could scar the fiber when he's loading it on the machine and ruin every carton that's made from the roll. But this isn't a major problem! We've got other more important things to worry about. We should be thinking about little projects that we can do while the machine is down. The problem is, what can we do until the engineers are through and not what we can do while they're there.

PAUL CLAYTON: What about the sealer?

FRANK RICHIE: Well now, I was just looking over the progress report to see if we have touched all the bases . . . this problem with the variation in the size of the cartons is one we have to do more thinking about. Tom Vycal thinks it's the cutter that's causing it.[2]

PAUL CLAYTON: The cutter doesn't make any difference in the carton size! I know it! I've been telling you, it doesn't make any difference. There's no need to make any tests, I can tell you right now.

FRANK RICHIE: But if we go ahead and change something else and they still come out too small, what then? [*Frank explained the tests would prove or disprove whether or not the cutter was affecting the diameter.*]

PAUL CLAYTON: The machine will be tied up and I won't get anything done—the cutter just doesn't make any difference!

FRANK RICHIE: That's what the tests will prove conclusively! When we're negotiating with Maroon we can then pull out the charts and data and show them why. Let's go—I've got other things to do.

The day following the argument with Frank Richie, Paul Clayton had the following conversation with Andy White:

PAUL CLAYTON: [*After talking about some administrative details.*] I don't know . . . it seems that you are all pushing me around so much, are you trying to push me into a corner? Don't you have any confidence in me?

ANDY WHITE: Oh, Paul, you know better than that, don't you?

PAUL CLAYTON: I guess so . . . I think I want to go back to automobile machinery. Take me off this project.

ANDY WHITE: Paul . . . who would take the machine to Norantha? [*The city where the dairy had contracted to have the cream machine delivered.*]

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[2] The cutter mechanism, consisting of a large reciprocating vertical blade and a feeder unit, apportioned the amount of raw material stock to be cut and then separated the piece prior to its being shaped. One theory held that the cutter synchronization was faulty, causing the completed carton to be under- or oversized.

PAUL CLAYTON: You've got other people, let Frank Richie do it.

ANDY WHITE: Paul, you know we need you.

PAUL CLAYTON: Yes . . . I know I've got only three years until retirement and you try to get rid of people when they're old.

ANDY WHITE: Now don't you talk that way—that's a bunch of rubbish.

PAUL CLAYTON: [*Pause.*] Well . . . I guess I blew my top—I'm sorry.

One of the most taxing problems to be faced on the cream machine project was that of determining the correct bearings for each of the machine's 70 rotating parts. In fact, one of the reasons the previous machine had failed in the cheese factory was that it had contained bearings that rusted under the extremely damp factory conditions. Greg Watson was determined not to have this situation recur.

Watson, upon returning from a trip to Norantha with a preliminary estimate of the situation, told the case writer:

"I think we've got a real problem. The dairy is literally dripping with water and I don't think we've taken this into account in our redesign. Maybe it's my fault in not making it clear, but somebody should have known that the machine would have water running around. Sid Ball [a bearing specialist from the engineering section who had consulted on the cheese machine and was presently being used for technical assistance on the cream machine] is the one that really goofed. As far as that is concerned, the whole team has let me down. Now we have to go through and check each of the bearings to see if they are moisture resistant. I'm getting pressure from Maroon and Vycal, who want to make sure we don't miss a bet. When ice cream starts to come out of that spout it can't wait for cartons—they've got to be there." It was imperative that the machine operate continuously without interruption. One of the machine's distinct advantages was that it could manufacture cartons in the same amount of time required to fill them. Thus, it was possible to link the forming machine directly with the filler and eliminate costly storage and transportation of empty cartons.

Watson's remedying action was swift and decisive. Since the day of his return from the visit to the dairy had coincided with Frank Richie's and Andy White's vacation period, Greg went directly to the assistant manager of Group 10, Ralph Larson. Mr. Watson directed him to reassign Stan Lomax to the cream machine project with the specific job of reviewing every bearing in the machine for its susceptibility to rust. He told Larson that he thought that those bearings which could be replaced quickly under normal dairy operating conditions did not have to be moisture proofed. That is to say, Watson thought the dairy would allow the machine to be nonoperative for a short period of time while a minor bearing was being replaced, but would probably dislike tearing the

machine down to fix a bearing located in a strategic mechanism which was hard to reach and fix.

Upon Watson's departure, Ralph Larson called in Stan Lomax and informed him of his new assignment. Lomax, one of Group 10's oldest and most experienced designers, had had considerable experience with bearing design work and had participated briefly in the design of the cheese machine several years earlier. (Review Stan Lomax's comments in Atlas Corporation [D].)

The following week Stan worked closely with Sid Ball, reviewing the machine's bearings, and consulting frequently with Paul Clayton and Frank Richie on the changes he and Sid thought appropriate. On one occasion, they went directly to Greg Watson to confer with him on the conditions the latter had observed in Norantha.

Ten days after Greg Watson had initiated the bearing review, a conversation took place outside Andy White's office. The discussion began when Stan Lomax and Sid Ball approached Frank Richie to inform him that they had computed it would take 24 designers four weeks to redesign the parts and install the new bearings that they thought the machine required. After conferring briefly with them, Richie called Andy White and asked him how they might go about obtaining the extra personnel.

ANDY WHITE: Really, we don't have the time or the money for it—but it's not the men that bother me so much as the number of weeks. Four weeks is taking us right down to the wire. I guess all we can do is see where we can cut some of it out.

STAN LOMAX: We've got it pared down to the bare minimum right now.

ANDY WHITE: Let's call down Greg and see what he says. [*The group discontinues the discussion briefly until Greg Watson arrives. Andy speaks directly to him.*] Lomax and Ball say that they figure that it will take at the very least 24 designers for four weeks to get this bearing thing straightened out.

GREG WATSON: What! [*Angrily.*] I thought I laid the ground rules down and told you that I just wanted to worry about the bearings that couldn't be replaced in 30 to 45 minutes. I didn't want everything changed!

STAN LOMAX: I thought you were telling us something like five minutes to change one. That's why we had to change so many of them—besides, most of the old ones wouldn't last very long with the machine running continuously. How long do you want the bearings to last, anyway?

GREG WATSON: [*Heatedly.*] I don't care—five years—three months—just so they can be replaced, I don't care how long they last. Come on out to the machine. I guess I'll have to show you what I mean. [*Group moves*

over to the machine.] Now see this bearing [*pointing to one.*]. It needs to be replaced because it would tear up the belt if it froze up and would be hard to get to. But take this bearing [*points to another*]. Anyone could take it off and replace it in thirty minutes. You see what I mean?

Sid Ball: Where did this thirty minutes come from? I thought it was less than that. Five or ten minutes.

Stan Lomax: That's what I thought Greg said.

Greg Watson: Now look, some bearings are going to be easily changed, some are going to be difficult. We have to pick out those which are a potential troublemaker and change them now so that they won't be in a bind when the machine is operating. Now tell me, am I crazy? Is there something wrong with my argument?

Sid Ball: The way you painted the picture to me it's going to be plenty damp in the factory. What's more, they will probably be hosing down the ice cream making machines with hot water and some might get on our machine. That means every bearing has to be waterproof.

Greg Watson: Yes, yes I know, all I need is a little judgment on this. I don't have a million dollars to spend and I've got to have this machine ready to go by November 1. Now let's get going.

The group dispersed. Watson, White, Richie and the case writer walked back to Andy White's office. The following conversation took place.

Greg Watson: Frank, you're going to have to take hold of this thing. Lomax and Ball have stampeded into this changing routine. You've got to watch over them. We need some supervision on this or both of them will go hog wild!

Frank Richie: [*Rather slowly, in a muffled voice.*] I personally believe that I wouldn't change any of the bearings. We've got a lot bigger problem than bearings to worry about before this machine is ready to go to Norantha.

Greg Watson: Well, if you believe that way strong enough, we won't do anything. But I'm warning you. You're going to have to fight Lomax and Ball. They're all hepped on this changing now. They'll tear your hide into little shreds if something goes wrong with the bearings while it's at Norantha.

The same day the case writer asked Frank Richie about his reaction to the episode between Greg Watson and the cream machine project team.

"I'm not much interested in this whole bearing affair. It's Greg's baby—he's the one who started it. Frankly, I think we have more pressing problems to worry about. I guess it's more of a political move than anything. Maroon is putting a lot of pressure on us, and Greg feels we should comply. Generally I'm unconcerned and apathetic."

Later in the afternoon the case writer had occasion to talk to Greg Watson about the bearing problem. Andy White was also present.

"I don't know why I should be making decisions like I did this morning. I'm really in no position to make a good one. But Sid Ball is like an old woman—so is Stan Lomax. That makes two of them. They want everything just so. They get to talking with one another and the job gets bigger and bigger. Pretty soon they're in over their heads. Sid Ball is some politician. What a backslapper. Right after I set Stan Lomax to reviewing the bearings, he came up to me and said I was right about the bearings, that we would have gotten into trouble if I hadn't suggested a review. Do you know we lean on engineering as a crutch, relying on them to make the decisions for us. I don't know why we didn't hire our own engineers in the first place. We can't rely on Ball—he stampedes.

"We've got a black eye from the cheese machine fiasco when the bearings froze up. Vycal is a strong technical man, and when he suggested that we look into the same problem on this machine, I had little choice. You know—we don't have a strong enough technical man to stand up to Vycal and talk his language. That's why I asked Sam to check out each bearing. I told him today the same thing I told him when he and Ball came to my office five days ago—to review each bearing and be sure with *reason* that the machine won't break down before too short a time. I'm trying to get them to buy a philosophy of reason. I can't tell them exactly what to do on each bearing. They'll have to make their own decisions. First, I told them to review each bearing, expecting that they would take it from there. But no—I had to come down and tell them exactly what I wanted. I shouldn't have to do that.

"This is where Ives' project supervising system should work—tie one man down to make the decision, to coordinate the whole project. This problem should have never reached me. A Frank Richie or an Andy White should have made the decision, but they took the easy way out and called me down. You see, it's a perpetuating cycle; next time they will call me up again, being more reluctant to make the decision than before. But this time it took less time to make the decision myself. We are pressed on this project and we can't afford to waste time or make the wrong move. I tried to take the smaller ones, the less important decisions and give them practice in making them.

"I went up to [vice president of research] to get this whole business straightened out between Maroon and us. I thought that he could set up some general arrangements with them so maybe Richie and Vycal could do all the deciding. You know what he told me? Work it out myself—after all that's what I'm getting paid for—to make decisions—work out the situation."

Stan Lomax's reaction to Greg Watson's plea for a less ambitious bearing change was as follows:

"Frankly I would like the machine to be done right, which would mean changing 120 of the 140 bearings. I guess we will make the changes on the next model, and we are just letting this one get by. But I still think this is going to run 24 hours a day, seven days a week—four to six months continuously. We had better build all the preventatives right into the machine now. In my opinion you can't be too careful with all that water and moisture around the machine. This cream machine is a lot different from automotive machines—it's got to run continuously under a lot different factory conditions. The way it stands now, I don't think the bearings will hold up."

The bearing problem was not the only disrupting element affecting the group's cohesion and progress. In fact, Frank Richie was not alone in his opinion that the project was growing "like Topsy." Greg Watson shared similar feelings, and these feelings often impelled him to intercede in order to relate the various aspects of the project to a common program. Moreover, time was running out, and Watson was extremely anxious to use every method he could invent to facilitate progress. Typically, he would call the team members together in Andy White's office and review their progress. Several days after the bearing incident, he assembled the cream machine personnel for a conference. The following conversation ensued:

Greg Watson: Now what's being done on this variation in the size of the carton? It looks to me as if the synchronizing mechanism is causing all the trouble—let's look at it.

Frank Richie: I don't know about that. The engineers are checking it out right now with their test. I want to wait and see what they come up with before we do any changing.

Greg Watson: You're the project supervisor.

Andy White: Why don't you take it off the machine and take a look at it?

Frank Richie: It's just too much work to take the whole cutter mechanism off and put it on again—besides, we haven't any way of setting the synchronization when we do get it off.

Greg Watson: What! We can put a couple of designers to working through the problem this afternoon.

Frank Richie: [*Dejectedly.*] I don't know. Who has all the answers anyway?

Greg Watson: [*Smiling.*] I'm just being facetious.

Frank Richie: We don't have to make any moves on this for another month, when we have to tear the machine down anyway.

Greg Watson: Well, I would like to know what's going on. Let's get the engineers down here and find out what they're doing!

A representative of the test engineers is called in to explain the test procedure and tentative results.

GREG WATSON: [*As the engineer leaves the room.*] Oh, by the way, Frank, how do we get the timing straight if something should go wrong when it's out in the dairy? We've got to be thinking about that.

FRANK RICHIE: It's relatively simple when we have the shaper timing to work from. I guess we could send along a couple of gauges with the machine to help them out.

GREG WATSON: Is there anybody making one now?

FRANK RICHIE: No.

GREG WATSON: Do you have a list of things you have to do?

FRANK RICHIE: Someplace, I guess.

ANDY WHITE: Frank, you gave it to Ralph Larson [*Andy White's assistant*].

GREG WATSON: [*Heatedly.*] That's your working paper not his!

ANDY WHITE: [*Points to his head.*]

GREG WATSON: His head is not good enough. Look, we've got some real problems. Take for example the sealer. That's not working at all and here you're not watching over these things.

FRANK RICHIE: [*Interrupting Greg.*] Now you're going too far; we've got that under control.

GREG WATSON: All right, what about the fiber? Are we going to have enough to run some cartons off when we go to test?

ANDY WHITE: Where's Gordon Kingston?[3] [*Gordon Kingston is called into the office.*]

GREG WATSON: How much fiber have we got on order?

G. KINGSTON: Enough for 2,000 cartons.

GREG WATSON: That's not enough! That's only ten hours' running time, and we need at least enough for 70,000 test cartons.

G. KINGSTON: We've got enough for 400 here now. If my second order for 1,600 is going to be delayed because of a breakdown at their plant, I really don't know when we're going to get the rest.

GREG WATSON: Not having enough fiber has hurt us in making test runs in the past, and I don't want this to happen again.

G. KINGSTON: I really have some doubt in my mind whether we wanted the one-ninth-inch size we had been using because it looked to me as if the cartons were all off size, so I ordered one-eighth stock. We're having a little trouble running the thicker stuff.

ANDY WHITE: Paul said the thinner material was all right and even made better cartons.

FRANK RICHIE: You can't make a statement like that unless you compare the cartons or at least make some tests on them.

GREG WATSON: I don't care what the thickness or what the reason is—Gordon, I've got to have some fiber here immediately!

[3] Gordon Kingston, the packaging specialist, had as his primary responsibility the liaison with the firm that was manufacturing the fiber material from which the cartons were made.

G. Kingston: You just can't create it.

Greg Watson: I want $29,000 worth of fiber here eventually. We've got only 20 percent now. When are we going to get the rest?

G. Kingston: It's my fault for holding up the order. . . . I was undecided which thickness to order.

Greg Watson: Can we get the one-ninth-inch stock sooner?

G. Kingston: Yes, I think so.

Greg Watson: I haven't a thing unless I have something to test with, Gordon, I want you to make some phone calls before you do anything else and make sure we get some fiber here! By the way, what sort of a gamble am I taking with the thinner stock?

G. Kingston: We refused it before.

Greg Watson: Then you talk with those people and make sure we've got some fiber coming. This whole business has held us up.

Frank Richie: I've got a suggestion—to help cut out all this bickering—why don't we put down all our work needs and plans on a schedule so everybody else will know what the other is doing. Then we could see if everything is getting done. [*Gordon Kingston leaves.*]

Greg Watson: You're the project supervisor. That's your job—do it!

Frank Richie: You're just a little bit bigger wheel than I am, and you can put on the pressure. I've talked to Gordon more times than I care to remember, in long drawn-out conversations, and I felt we were straight on this, of his telling me what's going on, but I'm still in the dark as to what's going on. He's got to tell me, I can't keep hovering over his shoulder.

Greg Watson: Just try to get along with him. . . . Let's go on to something else. What about the carton size? We should have some exact specifications as to just what we are heading for.

Frank Richie: I've got an outline.

Greg Watson: Who by?

Frank Richie: David Folk and Phil Singer.

Greg Watson: Did Kingston see it?

Frank Richie: No.

Greg Watson: That's what I've got Gordon here for. To check out specifications we don't know anything about. I want Gordon to jump in on this and see it through.

Frank Richie: We don't even know whether or not to use a fiber or metal bottom on the cartons. Isn't it about time we decided on this before we go hog wild on specifications?

Greg Watson: Damn right! Get Gordon back in here. [*Mr. Kingston is called back into the room and Mr. Watson continues.*] I want you to check over these carton specifications and get them set right now.

G. Kingston: You don't have enough information to set any specifications. How can you write any carton specifications when we don't even know which bottom to use?

GREG WATSON: All right, I'm making my own decision. Until somebody shows me I'm wrong, we're going to use the fiber bottom. Now, Gordon, I want you to sit down and make some carton specifications. [*Kingston leaves.*]

GREG WATSON: [*To Frank Richie.*] Gordon is harder than a sleepy Mexican to do business with. Will you for gosh sakes learn to work with him so he won't slow us down any more. Now, where do we stand on the bearings?

FRANK RICHIE: Since you told them to cut out most of the big changes, Stan Lomax says that we should use cadmium coating on the ones we already have. It will hold us up a bit because we have to send them out of town for the cadmium bath.

GREG WATSON: Are you buying that weak answer from Stan?

FRANK RICHIE: This is what you wanted.

GREG WATSON: For gosh sakes, can someone take them [*the bearings*] under his wing and be sure they're back here in time? Frank, I need a little thinking on this—time is very important.

FRANK RICHIE: [*Angrily.*] I haven't got enough time to check out everything a lead designer has to say!

GREG WATSON: You can't take his word for it!

FRANK RICHIE: Then you must be in accord with all engineering tests on the other problem.

GREG WATSON: Frank, my boy, I'm in accord with anything you are. I'm just trying to help you out. Although I would be a little ashamed I wasn't thinking about a gauge for setting the synchronization. I would be pushing the designers to make you one up. You might even press old Andy White for a man to help you out.

FRANK RICHIE: Who's right or who's wrong on this—no one has the answer. You ask Paul and you get one answer, the engineers another.

GREG WATSON: Well, I'm going to make this decision myself—Andy, I want a designer for two weeks; take him from another project and have them make an adequate gauge—so that any two-bit machine operator can get the timing correct.

FRANK RICHIE: You'll probably have to knock the whole program out when the engineers find out that the cutter mechanism isn't causing the trouble.

GREG WATSON: It's my decision and I'm going to at least get started.

After the cream machine project team members had left the office, Greg Watson described to the case writer the kind of problems he was experiencing with project supervisors such as Frank Richie.

Watson said, "The reason we're having all this trouble haggling over little things is because the men aren't making decisions when they should. I guess we are caught thin when it comes to good supervisors. We've grown so fast that it's pretty hard to keep up with finding good leaders.

How do you train good supervisors? This is what I want to know. We are caught in a real bind, and we have to produce on good projects without the people with position experience to make decisions. What I told Frank was two thirds kidding and one third the truth. But he's just not making decisions the way he should. Give him another four years and he'll have the world by the tail, but right now he hasn't had any experience.

Watson continued, "Gordon Kingston is an old woman trying to make decisions by himself. I could kick him in the seat of the pants for holding up that fiber order without telling someone the story. He's worried about a $200 roll of fiber when the whole project could flop when we don't get the material to make our test run. Gordon's got the information, but Frank or Andy just won't take the time to draw it out of him."

Later Frank Richie commented to the case writer: "Greg was just reviewing things I believe were unimportant, superficial, but you can't expect much more from his vantage point. I don't know why he is so anxious to get the fiber here—we won't be using it for another month."

In general, Gordon Kingston's relations with the rest of the project team members were rather strained, and as a consequence, Kingston usually confined his communication with them to matters that were absolutely necessary. Moreover, he was left out of many of the project team's informal conferences and was rarely invited to the formal meetings. The rift between Paul Clayton and Kingston had been so violent that they refused to talk with one another. Clayton frequently told Kingston that his background was insufficient to qualify him for making suggestions concerning the machine design. The latter frequently replied to this attack by deprecating Clayton's ability and knowledge of the packaging field as a whole. Kingston's responsibilities kept him away from the laboratory most of the time, or the situation would have certainly become much worse.

Frank Richie also found Kingston's behavior disturbing, particularly his habit of taking his orders exclusively from Watson, avoiding Frank's suggestions almost completely. In addition, Kingston's work on the carton design frequently took him on much-envied trips to packaging conventions and conferences which other team members were not entitled to attend. Phil Singer commented, "Gordon's way out in left field. As far as I'm concerned he's just looking out for himself. What makes me mad is that I've never been to a packaging show and he's been to three already. Not once did he write a report for any of us to read—all for his own personal benefit. He feels he has to go directly to Greg, but if I did something like that I couldn't look the rest of the team members in the face."

In an attempt to circumvent Kingston's obstinacy, Frank Richie had, during one of the latter's absences, assigned two other designers, David Folk and Phil Singer, to begin a more systematic review of the carton specifications.

Frank Richie described his problems with Gordon Kingston to the case writer as follows. "When he was first assigned to the project, he didn't accept me as his boss, although it was made quite clear to him that he should be working for me. But he wanted higher status, so he kept going to Greg for all his assignments and problems. I've tried to reason with him—talk to him—anything, just so I could at least find out what he is doing. That review session with Watson was ridiculous. He should have told me why he was holding up the order. Watson keeps telling me to take him out on a trip, sit down over a drink, and talk things over. In the first place I don't know of any trip and secondly, I wouldn't know what else to say—I've reasoned with him in every way I know. There's just a big difference between a section head talking to him and a project supervisor doing the same thing. The only way I found to keep things under my control is to assign the work he is supposed to do to people who will at least listen to my suggestions. He might be a packaging specialist, but he is no test engineer. Setting these carton specifications is one instance where you need some tests. Anybody can sit back and give opinions like Gordon was doing, but we need people to prove whether something is right or wrong."

By the middle of August, 1960, the cream machine project team was entering the final phase of its activities leading up to the hoped-for completion date of October 1. Most of the team personnel viewed the final six weeks as an extremely crucial period, since much of their work to date would be put to practical test as components were incorporated into the machine's final design. With the exception of two weeks, when various minor changes had been made, the prototype machine had been kept running in order that the engineers might complete their tests; but starting in the last week of August, Frank Richie had scheduled the machine to be completely torn down so that all of the design changes could be made. Also, taking his cue from Greg Watson, Richie had laid down a detailed plan covering activities necessary to finish the project on time.

Frank Richie expressed some doubt as to whether or not the project would, in fact, be completed as scheduled. He believed he faced some formidable personnel problems. He said, "I'm not at all sure we're going to be done on time. A lot of things are still left hanging, and I'm not sure how long it's going to take to get them completed. I guess my biggest problem is that some of the designers are apprehensive about scheduling their work into the plan I've mapped out. Instead of going around once a week or even once every two weeks like I used to, I'm now talking to them three or four times a week, and this is being met with some strong resistance. They just don't seem to want to move or take into account the overall project. Greg can come down and tell them what to do and they'll listen to him, but they won't pay much attention to me. Paul and Gordon are my biggest headaches.

"Last week I wanted Paul to finish up the design of the cutter mechanism he had started while the machine wasn't operating. He said he

couldn't do a thing while the machine wasn't running, and he gets so anxious while the engineers are around when it's going. Now he wants me to wait until we're almost down to the wire before he works on the cutter.

"Most of this time Paul is racing around from designer to designer. He wants to be in the area where all the activity is. If we're doing a little work on one particular part, you can be sure Paul will be right in the middle of it. He thinks he can come up with a better idea than the designers and the engineers, rather than going off and working on something else that is equally pressing. The cutter problem for example. We're going in one direction and Paul's taking another. The engineers are making an adjustment in the shaper which will correct our carton variation, and Paul's working on the same thing using another method. I invited him to a meeting with the engineers this morning and he wouldn't even come along. He said, 'I don't have any faith in that program.' For what reasons he couldn't say. Paul's philosophy is, get those engineer people off the job so I can fix the machine, when really, they are the very people we need.

"Paul is always complaining about so many loose ends—that he doesn't have everything under his thumb. But he's not expected to have everything under his thumb. In my book the lead designer has got the technical responsibility, but he's not called upon to worry about scheduling. Every time anybody is assigned to the project, Paul gets all bothered and in a big huff. I guess he sees some of the glory and prestige taken away from him—in this respect he's immature—kindergarten behavior. Of late there's been so many people he can't do anything about it though. He wouldn't even talk to me for a couple of days last week because I spoke to a group of Atlas salesmen about the cream machine project when he thought he should have."

When the casewriter spoke to Paul Clayton, he found a different point of view. Clayton said, "The final result of the engineering tests just prove what I've been telling them all along—the shaper was causing all our trouble and not the cutter mechanism. I've been telling them this all along. I'm making a new type of shaper now so we can get this whole thing straightened out. The engineers took six weeks to make their tests, and in the meantime, I couldn't do anything in the way of trying out my ideas. Can you imagine taking that long to take some tests? I assembled the whole machine in eight weeks. That's the only way you can see if an idea works—design it, make it up, and see if it works.

"Everybody has ideas and the engineers want to do it this way or that, but you can't tell if it will work until you've made it up. The engineering test could go on forever, but how could anybody know what idea worked? It's awfully hard to get your point across now. A person shouldn't be so stubborn that he won't listen to me. It might take some time to prove my point, but they should listen to me. That's the only way

you can see if a person is right. Build a model and see if it works. Why, before, when an inventor knew more than I, I respected him for it. We would pass information back and forth, but I knew he knew more than I. When I was an apprentice you didn't give any back talk, you took it. They should listen to the experienced man and give him a chance to prove his point. This is the way we do it in automobile machines; that's the best way to see if you're right. Andy says that I should go ahead, but you need a man who is receptive to tell him something. I've been in this group a long time and practically all the people have come up and asked me at one time or another a question about something.

"I have a feeling I don't have control over the project like I should. There are so many people working on the project, and I don't know what each one of them is doing or what's going on. I can't direct the people unless I know what they're doing. Frank Richie is project supervisor and I'm the lead designer and I think we should know what is going on. Maybe I've been a little obnoxious in trying to find out what's happening, but no matter what the conference, I think I should be in on it because it might relate to what I am doing. When I came back from vacation, Frank Richie was calling all the shots. He should have filled me in on what had been done while I was gone. If they expect me to work, they've got to let me in on these technical discussions. Watson is always directing the talk to Richie—maybe this is the way management wants it to be. Those people up there at the higher echelons are probably saying that the project supervisor should be the one that directs things and has the say on what changes are made.

"I usually get along good with people. Why, during the war I was lead designer on some of the big gun projects where I traveled to New York, Baltimore, all over. The company got reports back saying that I was doing a fine job. The projects always turned out satisfactorily. I don't know what's wrong with Frank Richie; it's hard to get under his skin. They said that he was afraid of me. But I never want anybody to feel that way about me. Andy says that I was supposed to crack his shell by going up to him and seeing if there was anything for me to help him out with. This really galled me; I shouldn't have to do that. I've got to admit that I've been pretty frustrated for the last three weeks, and I don't know what is wrong. There doesn't seem to be any direction—something is missing. You've been down here for some time and know what's going on. Is there anything wrong with the way I've been acting? Should I change my way? Because if there is something wrong I'll change my way.

"I just feel an undercurrent that something is wrong. No one will come out and tell me what it is. I don't even know where Greg stands on this—he seems dissatisfied, but I don't know why."

Chapter 32

CONTINENTAL ALUMINUM COMPANY (A)*

INTRODUCTION

THE CENTRAL research laboratory of the Continental Aluminum Company (CAC) differed significantly from the many other R & D Divisions in industry in a number of important respects. In the first place, it was large in terms of both professional employees and annual budgets. (And yet its research budget came to less than 1 percent of the company's sales.) Second, the company as a whole manufactured and sold a broad range of products, as a consequence of which its R & D activities extended to more areas of science than those of many other companies. Third, it had been operating as a full-fledged laboratory years before many other laboratories had employed even a single man on R & D work. Fourth, it included in its activities a substantial proportion of research which would be classified as basic or fundamental in character. Lastly, CAC operated a number of manufacturing divisions, each of which employed many technical and professional people in production activities and some of which had R & D laboratories of their own. These differences are clearly related to one another and, to a considerable extent, to the sales volume and product lines of the respective companies. The scale of operations of CAC enabled the laboratory to engage in activities which were clearly not feasible in many other companies.

A number of the distinctive features of research activities in CAC, however, manifestly were not simply a function of size or a diversified product line. The point here is that some features of R & D management which stand out in CAC are not simply a function of size but rather stem from differences in management philosophy. It is for this reason that this series of cases provides a potential means of illuminating problems in the relationship between scientists and management.

In 1953, the central research laboratory was under the direction of Dr. Ralph Zanke, a vice president of the corporation and recognized nationally for his own scientific achievements as well as for his work as head of a laboratory which enjoyed an international reputation for excellence. The research activities of the laboratory were divided (primarily by functional

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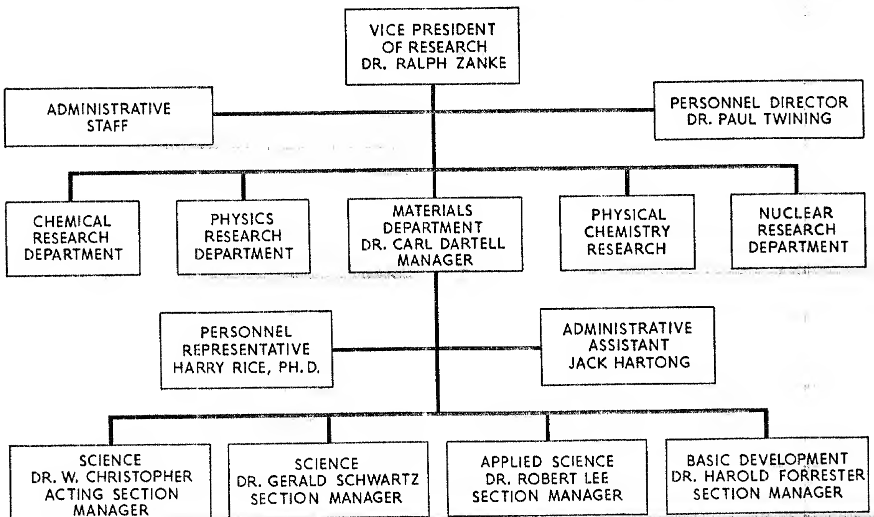
specialty) among five operating departments, each under a manager who reported to Dr. Zanke. Also reporting to him was a fairly large staff charged with certain administrative responsibilities for the laboratory as a whole (see Exhibit 1). These responsibilities of the staff included budgeting, personnel administration, patent administration, provision and maintenance of facilities, purchasing liaison with other divisions of the corporation, and certain planning and evaluating activities.

In the course of a day-long interview with Dr. Twining, personnel

Exhibit 1

CENTRAL RESEARCH LABORATORY, CONTINENTAL ALUMINUM COMPANY,

PARTIAL ORGANIZATION CHART



NOTE: This chart is an attempt by the observer to note the relationship between various persons named in the data. In actual fact no such chart existed within the laboratory. Names and subject areas have been disguised.

director of the central research laboratory, the researchers were given information which can be grouped into three general areas:

1. *The Central Research Laboratory in Relation to the CAC Organization.*
   a) The central research laboratory was a separate division of the corporation, and Dr. Zanke, as vice president of research, reported directly to the executive committee of the company.
   b) There were several smaller laboratories in the company. These reported to supervisors within the manufacturing divisions. Dr. Zanke had no direct responsibility for the management of these divisional laboratories.
   c) Four general types of research were carried on throughout the

CAC organization. The first was scientific research, and almost all of this was done at the central research laboratory. This was the type of work which has been variously called "basic," "fundamental," "pure," or "blue sky" research. The second was "applied" research. Some of this was also done at the central research laboratory, but an attempt was made through the efforts of liaison scientists to carry on much of this type of work at other laboratories. The third type of research was development work; this was done largely at the divisional level although, once again, the central research laboratory did engage in development work to a limited extent, particularly when an unusually difficult problem was involved. The last type of research, engineering development, was carried on entirely in laboratories other than the central research laboratory.

2. *Policies of Research Administration at the Central Research Laboratory.*

Dr. Twining summarized a number of policies which he believed were important to the atmosphere of the central research laboratory. These were as follows:

a) *Relationship with Operating Components.* One of the early directors of research of the central research laboratory had often expressed his idea of relationships with operating divisions of the company as follows: "Never let a cry for help from operating divisions go unanswered." Dr. Twining felt that the philosophy expressed in this statement had not really carried through to the present, largely because of the creation of other laboratories in the company which had not existed at the time the policy was first proclaimed. The central research laboratory still provided a measure of scientific support—particularly consulting service—to all divisions of the company. The emphasis in recent years, however, had been to conduct most of the "trouble-shooting" and much development work within manufacturing divisions rather than at the central research laboratory.

b) *Free Exchange of Information.* It was believed that free exchange of scientific information both within the company and with scientists elsewhere was essential to the company's scientific research.

c) *Cooperative Effort.* Believing that scientists should be able to turn to their colleagues for help in solving their scientific problems, management explicitly encouraged the concept of cooperative efforts within the laboratory.

d) *Interesting Work.* Management believed that it had the responsi-

bility for seeing to it that a scientist was enabled to do work which interested him as an individual.

e) *Freedom of Choice.* Given the opportunity to do work in a field which interested him, the scientist also should be permitted to choose the specific projects on which he would spend his time. He should be free to go about his work in his own way. Management believed that this type of freedom led to a mutual stimulation among the scientific group at the laboratory which could not be fostered in any other way.

f) *Approbation of Scientific Peers.* Management believed that to a scientist the approval of his colleagues in science was more important than the respect and admiration of company management. As a means of assisting technical personnel to achieve recognition in the scientific world, management encouraged them to publish papers and take part in scientific meetings.

g) *Prompt Publication.* Management believed that technical papers, once completed by a scientist, should be published as quickly as possible. Accordingly, the policy was to avoid any postponement of publication for competitive reasons except in unusual cases. ("Quickly" in CAC meant within a month or two, whereas in some companies it often means within a year or two!)

h) *Financial Security.* It was considered important that the scientist be rewarded financially at a level which would allow him to provide for the security of his family, educate his children, and maintain a satisfactory position in his community.

i) *Atmosphere Free from Distraction.* Finally, it was believed that management should attempt to minimize distractions as one part of the effort to maintain an atmosphere conducive to research. The reference here was specifically to administrative detail and red tape.

Commenting on these policies, Dr. Twining stated that they were very much aware of the traditional democratic spirit which had always existed in the laboratory and wanted to do everything possible to preserve it. He said, "People knew each other in the old days," referring to the fact that the laboratory had doubled in size in the past four or five years. He was talking particularly about the closeness of the executive group in the laboratory. He indicated that because of promotions or transfers of personnel from the central research laboratory to the smaller laboratories which had been set up in the company in recent years, it became more and more difficult for laboratory management to know their people as well as formerly. He also used the words "high morale" in describing the current situation in the laboratory. He felt that this was a result not only of the philosophies and ideals of past directors of research but also of the unusually fine facilities at the laboratory.

3. The Current Organization of the Central Research Laboratory.

The scientific work was divided among five operating departments. These departments generally were organized more or less along functional lines; two of them were staffed mainly by physicists and a third primarily by chemists. The materials department under Dr. Dartell was somewhat unusual in that, because of its concentration on the properties of materials, it cut across functional lines to a greater extent than the other departments.

As noted above, each of the operating departments was directed by a department manager who ordinarily had reporting to him two or more section managers. The supervisory organization of the laboratory in other words consisted of only three levels: Dr. Zanke, as director of research, the department managers, and the section managers who reported to them.

Below the section managers there were only the two formal titles—"research associate" and "research assistant." Dr. Twining stated that the administrative heads of the laboratory did not want to introduce a technical hierarchy. They felt that men should be free to get ideas, gather around them a group of equals to work on them, and then have the men who got the ideas act as informal group leaders as seemed appropriate. They regarded the emphasizing of titles as not desirable. Laboratory management believed that titles and labels were unnecessary when technical men could publish the results of their work and thus gain recognition from other scientists. In effect, the prevailing ideal in the laboratory was that men should be known by their deeds rather than by their organizational titles.

In addition to the five operating departments, there was a sizable administrative staff, as mentioned above. This staff was divided into two departments, which reported directly to Dr. Zanke. Dr. Twining said that there had recently been a trend toward making the operating departments more autonomous insofar as administration was concerned. He pointed out that the department managers as a group, when appointed, had little training for or interest in research administration. To help them handle their administrative problems, personnel representatives were now being selected out of the various departments. In appointing these personnel representatives, an attempt had been made to select scientific people who had shown a particular interest in personnel work as well as some ability to handle it. Dr. Twining believed that such men should have a sympathetic understanding of scientific people and also be willing to help with whatever problems professional people might bring to their attention.

A Meeting with Dr. Carl Dartell

The observers first met Dr. Carl Dartell, manager of the materials department, at a luncheon attended by Dr. Twining and several of the

other department managers. This luncheon took place on the occasion of the observers' first visit to the central research laboratory some six months before one of the observers returned to observe the activities of the materials department.

Before the luncheon Dr. Twining had told the observers something about Dr. Dartell. Twining had said that Dr. Dartell was the only one of the five department managers who might be considered a "real administrator." In addition to having an excellent reputation as a scientist, Dr. Dartell was also interested in administration and apparently was regarded by laboratory management as more competent in this field than the other department managers. He had been manager of the materials department for about four years, and he had done an excellent job, Dr. Twining said, in expanding and reorganizing the department during that time.

Dr. Dartell was relatively young—in his late thirties. Soon after the group seated themselves at the lunch table, prompted by a few questions from the observers, he began to talk about research and the philosophies which guided his own administrative thinking. He spoke very graphically and very forcefully. His lively interest and his strong emotional identification with his subject made it easier for the observers to accept what they would otherwise have regarded as a somewhat blunt and almost abrasive personality. Dr. Dartell's remarks are paraphrased in the following excerpts from the observers' notes:

A Philosophy of Research Administration

Dr. Dartell separated research personnel into three overlapping groups: (1) people occupied with "pure research," (2) people working on the application of the results of pure research, and (3) a development group. He felt that the personnel of each of these groups was oriented entirely to the scientific community—that they had neither need for, nor appreciation of, the values of line administrators or company management. All they wanted was to be left alone to work in favorable circumstances on the problems in which they were interested. For them the necessary satisfaction and the achievement of status would be satisfied by good scientific work. On the other hand, he felt that the interests of the people at the development end of his scale were closely related to those of the company generally and particularly to the administrative hierarchy of the company. These, he said, were the people most interested in getting into administrative positions as a means of acquiring status and getting ahead.

Dr. Dartell was very vehement in stating his belief that an understanding of the different needs of the three groups was vital to anyone in research management. He summarized his viewpoint by ascribing the different orientations of these people toward their work as follows: The scientific researcher is interested in "Why does this happen?" The man who is interested in applying the knowledge gained by the scientific

researcher says, "How can we take this knowledge and use it?" The man in development work says, "In view of its potential use, what products can we devise for manufacture and sale?"

In Dr. Dartell's opinion, when a research scientist came to work for CAC, the company and the research administrators were in effect making an agreement with him. They were agreeing to provide him with adequate facilities for his work, with satisfactory remuneration, with a wide range of interesting fields of investigation to consider, and, lastly, with the freedom to choose those problems on which he wanted to work and the manner in which he chose to investigate them.

So long as a man was technically competent, this agreement had to be kept or a scientific researcher would feel that his integrity was being violated, and he would soon leave the company. Dartell said that one could not judge research people on the basis of their positive contributions to company profit. They could be judged only on the basis of their standing in the scientific community, since this was the way they judge themselves. One reason that nontechnical administrators could not manage scientific groups, he felt, was that they did not understand this and tried to judge scientific people as they themselves wanted to be judged—by direct contributions to company goals.

He continued by stating that scientists communicated well only with other scientists who were close to their particular level of research. They could not readily skip a band in the research-development spectrum and communicate effectively with people who were doing work based on a point of view markedly different from that in which they were interested. In other words, the man working on basic research would find it very difficult to talk intelligently to a man in the product development field. As a consequence, the research administrator should provide intermediaries, he felt, so that the various groups could understand each other and could pass information up and down the line from the pure research personnel to the product development people. Dr. Dartell summed up by saying that the technical administrator's job was essentially to get the right man working on the right problem at the right time and in the right way.

Second Interview with Dr. Dartell

As a result of this initial contact with Dr. Dartell, the observers decided that they would like to observe at first hand in Dr. Dartell's department the application of his administrative philosophy. Some six months later, therefore, one of the observers made arrangements to spend two days in Dr. Dartell's department. The morning of the first day was spent with Dr. Dartell himself, and this conversation gave the observer further material on Dr. Dartell's philosophies of management as well as some information about the organization of the materials department. His notes on this interview read as follows:

Dartell started by discussing some of his ideas about the nature of research and how it had to be managed. He said that the difference between scientific research and the work commonly called "research" in industry lay in the purpose of the investigation. Science had to do with knowledge of nature. In contrast, industrial management often tried to impose specific commercial goals on the individuals in the laboratory. When this happened, there was no science, and furthermore, the goals of the organization were not met.

He said that the long-range goal of a technical laboratory was primarily to make money for CAC. In other words, company management invested dollars and hoped to maximize the return of the investment. The question was, how does management spread its dollar to get the maximum return? One opportunity for investment was in technical work. People in technical work, therefore, had to ask themselves: what are the critical scientific problems of the company? Management did not have the know-how to be able to select scientific problems wisely themselves. The technical people at the bench level and slightly above had to do this. In certain fields you found you needed more knowledge; so then you hired people who were interested in gaining more knowledge in these fields. Then you hired applied people to seek out technical applied problems—critical ones—and these were new problems which required new knowledge. Asking, for instance, for more knowledge of solid state physics would be a typical problem in the applied field.

Dr. Dartell drew a little diagram on his blackboard to show the basic organization of the materials department. He started off at the top of the diagram by pairing "science" and "understanding." The next level was "applied science" and "basic development." He said that in his department he had one group of people working on the critical scientific problems of the company, a second group of people working on the critical applied problems, and a third group of people working on the critical basic development problems. He pointed out that it was difficult for the people working on science to talk to the people working on basic development. Therefore, the group of people in the applied area served, among other things, as a communication link between the basic people and the development people. He said that project teams were not good and that you could not possibly accept the organization which he had set up if you favored the idea of project teams.

He went on to say that a man would be loyal to the company for which he was working as long as the company had a set of values with which he could live and did not demand that he violate his scientific integrity. The researcher said that it seemed to him that he (Dartell) believed very strongly in scientific work but the researcher wondered if all companies could afford to believe as he did. Dartell asked the researcher to give him an example where the kind of an organization he had set up would not be the best way of handling matters. The researcher answered by asking

Dartell what he would do if he had a small budget in the insecticidal chemistry field and wanted to make this budget do the most possible for him in a chemical company. He said that he would have one man investigating the behavior of insects (in other words, doing basic research in entomology) and then would hire two men to apply the knowledge that the one man accumulated. He felt that this would be a far better plan than to have three men applying the knowledge of other people. He pointed out that if your research people had to depend on reading the papers of the scientist that someone else hired, you were three years behind; but when you hired a scientist, you were probably three years ahead. Therefore, it made good competitive sense to hire your own scientific investigators.

Dr. Dartell amplified his stand on project teams by stating that in scientific research the effort must be individual, whereas in development work of the type being done in the laboratories of manufacturing components, the project teams would be useful. He said that you should bring new men into the fundamental research section—that this is where they wanted to start and where he wanted to place them. Then, as they gained experience and understanding, they would stay or move according to their interests and abilities down through applied work into development.

Dr. Dartell then outlined the organization of his four sections. Two of these were basic scientific groups—one headed by Dr. Gerald Schwartz, who was a physical chemist and had been at the CAC laboratory for five years. The other was being temporarily managed by Dr. William Christopher while the section manager, Dr. Allan Pike, was on temporary assignment elsewhere. In charge of the applied group was Dr. Robert Lee, who was a metallurgist and had been at the laboratory for two years. In charge of the basic development section was Dr. Harold Forrester, who had come to the central research laboratory less than a year previously.

Dr. Dartell then said that he did not like written reports. In his department, supervisors were responsible for knowing what their men were doing. They obtained this information by being in constant personal contact with their men. The only written reports he required were on completed work. He pointed out that he was particularly anxious for his men to publish technical papers, and therefore made an intensive effort to get them to do this. The publication rate in his department was now 1.3 papers per technical person per year, which he considered to be a very high rate. This ratio was based on research assistants as well as PH.D.s. Memorandum reports were used to present significant data to higher levels in the organization. These were apparently used infrequently, however, and it was up to a man to decide when to write them. Sometimes the CAC patent specialists wanted these memorandum reports as extra data after a completion report had been filed.

Dr. Dartell said he believed that most reports were trivia that kept

people away from their real work. He pointed out that twice a year they had a formal review with section managers as one means of keeping up with the work being done in the laboratory, and that he, himself, spent from 25 to 30 percent of his time actually in the laboratories talking with the people at the bench. The scientific section heads had their own laboratories, actually did research work on projects of their own, and therefore spent a little less time in other people's laboratories. Dr. Lee, who headed up the applied section, spent more time in his subordinates' laboratories than either Dr. Schwartz or Dr. Forrester.

Dr. Dartell went on to say that, after the first year a man was at the laboratory, he himself, as well as the man's section manager, talked with the man in an attempt to evaluate his performance and to counsel him on his future progress. Thereafter the section head talked to the man each year. During these sessions the section head would review the man's progress and his salary, although advances in salary were not directly connected with these interviews.

Dartell as a Research Administrator

Before proceeding to the other cases in this series, it is important to record certain facts and additional impressions and opinions about Dr. Dartell which were derived from the period which the observer spent at CAC. One thing the researcher observed was that Dr. Dartell appeared to have almost no social life which was not related in some way to his work as department manager. Four or five evenings a week he would invite to his home a number of men from the technical staff or visitors to the laboratory for cocktails and conversation. On such occasions much of the discussion focused on current managerial problems in the laboratory or ideas for future action in the department. In addition, when the solution to some problem involving people in his department proved unusually difficult, Dr. Dartell spent hours discussing alternative courses of action with his wife, whose ability to penetrate to the heart of such problems and make useful suggestions had obviously won his respect.

In his interactions with members of his department, Dr. Dartell was quite informal but rarely relaxed. He used first names in addressing all the people who worked for him. Most of them, in turn, used his first name in speaking to him. Discussion usually went in both directions, for Dartell was an intense listener, and on occasion there were quips and jests. At the same time, one could not describe the interchanges as free and easy. Dartell usually had a great deal on his mind, took his work very seriously, and was as diligent in soliciting opinions from other people as he was in expressing his own. No person could observe the interchanges for long without concluding that Dartell regarded the administration of the materials department as the most important thing in his life.

Dr. Dartell appeared not to regard written communication as very

effective. Evidently able to read and absorb what he read very rapidly, he worked only a few minutes each morning and evening on correspondence. The rest of the time he devoted largely to talking with people, individually or in groups. When he was in his office he was constantly interrupted by members of the staff who had something they wanted to talk about. The observer seldom saw him concentrate for very long on paper work. He seemed to regard his office as being primarily a conference room and a message center. A rough check indicated that Dr. Dartell spent approximately 80 percent of his time talking to people. Occasionally he would pick up his telephone and ask a man to come to his office. More frequently, he would leave his office—often without informing his secretary as to his destination—and spend several hours wandering around the various laboratories, talking to people about their work.

There were very few formally scheduled meetings in the department. On the other hand, Dartell called together a group of interested personnel whenever a problem came up that he believed would be worth discussing or when a member of the staff brought to his attention the need for a conference. Hardly a day passed without at least one such gathering. During the course of such meetings, Dartell presided with a rather tight rein and not infrequently manifested impatience. Although he seldom dominated the discussion, he disliked long-winded commentaries and frequently cut short the remarks of men who indulged in them.

In all his contacts with people, whether individually or in groups, Dartell was constantly asking questions. Sometimes he would answer his own questions; on other occasions he would very skillfully lead a man through a series of questions, winding up with a conclusion that Dr. Dartell seemed to have anticipated in advance.

On all occasions Dr. Dartell exhibited a quick mind and a high degree of verbal skill. Having his own views on almost any subject and being eager to get on with the discussion, Dartell was often abrupt with others. His own statements were incisive, and men who found it difficult to express themselves after an interview with the department head seemed to the observer to behave as if they had been through a rather difficult experience. However, one also gained the impression that Dartell, in spite of his bluff manner, was quite sensitive to the feelings and needs of the people who worked in his department. If a personal problem was affecting the work of any member of the staff, Dartell usually knew about it and on several occasions intervened when he thought he could be helpful.

Dartell's main concern was the overall effectiveness of his department. Doubtless he gave a good deal of consideration to the views of his superiors in the organizational hierarchy, but he exhibited no concern in that direction. Instead, in his daily behavior, he seemed to bend all efforts to stimulating, helping, and guiding the professionals as they went about their work. Anything that threatened the effectiveness of the department

in its research efforts obviously was of critical importance in his mind until the problem was solved.

Throughout the department there seemed to be a continuous atmosphere of subdued excitement. The observer did not describe the atmosphere of the department as tense, but he did note that everyone seemed intensely interested, curious, and quite absorbed in his own work or that of colleagues. It seemed to the observer that the behavior which he observed in the department was partly the result of high standards set by Dartell for technical activities and, in part, the consequence of Dartell's obvious dedication to the welfare of the department—expressed by him both explicitly and implicitly in his interactions with staff members.

The fact that the department was on the point of expanding into a new wing of the main laboratory building, and into another building as well, appeared also to contribute to a high morale which appeared to characterize the department as a whole. Everyone seemed to be fully aware that these expansions were part of a long-range plan to enlarge the activities of the materials department still further. Several staff members expressed to the interviewer the belief that this growth was directly attributable to Dartell's efforts to advance the work of the department and to the faith of the company management in his ability as a manager of research.

During the period in which the observer was visiting the laboratory, a number of Dartell's subordinates made comments about the department manager. Notes taken at the time include the following statements:

SCIENTIST: Carl is a very superior administrator.

OBSERVER: What makes him one in your judgment?

SCIENTIST: Well, he sets up a system, but he regards it as being tentative. He does not insist on holding to conclusions. He is willing to change. He is also very much aware of people. People are more important to him than the system. I feel I can learn a lot about administration simply by watching how he operates.

* * * *

RICE: Incidentally, Carl is the best organizer I have ever heard about.

OBSERVER: I gather from various things you have said that he uses the organization to serve the people instead of using the people to serve the organization.

RICE: That's right. Many candidates for jobs come here to look at us and think the way the place is run is revolutionary. I point out that we just follow good principles of management and carry them to a logical conclusion. If Carl hires a theoretical physicist, he sees to it that this new man gets to work the way he wants to. In effect, the company operations are a million miles away because the scientist is buffered from operating problems by the development section.

* * * *

MRS. DARTELL: Carl cares deeply about what happens in that laboratory. He cares so much that it makes him afraid. Every time he has to make a difficult decision he literally trembles. It is a terribly painful process for him, but he throws himself into it the way he does everything he undertakes. When I married him my friends said he wouldn't be alive ten years later, and at that time I was inclined to agree with them.

* * * *

SCIENTIST: You know, Dartell never did have an outstanding scientific reputation. He often published without sufficient data to back up his conclusions. He is primarily an idea man. He hates detailed experiments. Actually he is ideally suited to supervise research because, as an idea man, he can give the men lots of helpful suggestions which lead them to do the experimental work that he would be too impatient to do himself.

* * * *

SCIENTIST: Knowing Carl, you can imagine he makes fast friends or bitter enemies. I myself took an instant dislike to him and almost decided not to come to work here because of that. I think a lot of people are good to work for but hard to get along with if you are outside their organization. I have completely changed my viewpoint about Carl, but, of course, he has mellowed considerably.

OBSERVER: What do you mean by that?

SCIENTIST: I don't know what did it—probably no single experience. Probably it was an awareness that he was disliked by many people. He is becoming more tactful. I think he finds that he gets better cooperation by using tact. When you get to know the man, you realize his occasional sharpness is not a personal affront at all, but I know a lot of people in the company, or at least several people, who used to say they had a lot of trouble getting along with him. They now say he has changed. He was a controversial character, and he still is to some extent, even though everyone respects him for his ability. One guy once said to me that if he wanted to get a job in another company in this field of research he could always use the excuse that he couldn't get along with Dartell—that he would be understood and in good company.

* * * *

WIRTHEIMER: Carl has a fierce loyalty to his staff, and even if everyone else likes a man, if he doesn't, that man doesn't belong here. Dartell tells everyone that his is the best research staff there is. He really believes this, and it is important that he does. He couldn't believe this if the men did not get along with him.

Chapter 32 (Continued)

CONTINENTAL ALUMINUM (B)*

RECRUITING SCIENTIFIC PERSONNEL

As a result of the two days spent with Dr. Dartell and several of his subordinates in the materials department at the central research laboratory, the observer requested that he be allowed to return to the laboratory to spend a few weeks observing activities and interviewing various people in the materials department. This request was granted and the material that follows was gathered during this period.

During the first day of his observations in the materials department, the observer became aware that the recruiting season was in full swing. The time was spring of 1954, and men who had just completed requirements for their Ph.D. degrees from universities all over the country were coming into the central research laboratory to be interviewed for positions.

In recruiting young scientists, the managers of the central research laboratory enjoyed certain advantages over their counterparts in other industrial laboratories. The laboratory had exceptional research facilities, its salary scale was regarded as high, it was known in academic circles as maintaining a research atmosphere, and on its staff were a number of scientists who had achieved international recognition for their investigations. Two additional features were often cited as among the advantages CAC could offer. One was the fact that CAC was comprised of a number of manufacturing divisions, each of which employed large numbers of technical and professional personnel in both manufacturing and R & D operations. As a consequence, there was an unusual array of career alternatives open to a man who later wished to transfer from the laboratory to a different kind of work. Secondly, because of the wide diversity of the company's products, many areas of scientific research were potentially useful, and scientists employed in the central research laboratory could, as a result, choose from an unusually wide variety of research projects, according to their individual interests and capabilities. Such attractions led many of the outstanding young Ph.D.s from all parts of the country to come to the central research laboratory to be interviewed.

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Dr. Dartell had told the observer that they were hoping to hire 20 new men this year in the materials department alone. As a result, the observer's interviews with men in the department were often interrupted when the man with whom he was talking kept an appointment with one of the candidates visiting the laboratory. Dr. Dartell also made it a practice to talk to almost every potential recruit himself. At frequent intervals during the recruiting season, all the men who had talked to each candidate met and decided whether they wanted to make the man an offer.

The observer's first experience with a recruiting interview came early in the observation period during a day he had spent sitting in Dr. Dartell's office. A candidate by the name of Damon, who had already talked to a number of people at the laboratory, came in to see Dr. Dartell before leaving.

Dr. Dartell Interviews a Candidate

When Damon first came into Dr. Dartell's office, he said that he was amazed to find that there were so many top scientists working at the laboratory, and he also said that the equipment and the laboratory facilities were like nothing he had ever seen before. He went on to explain that he was not interested in basic research; that he was more interested in the development work under Dr. Forrester. He asked Dr. Dartell if most of the ideas for research projects came from within the organization. Dr. Dartell replied that 99.8 percent of the ideas for research came up from people at the bench and went on to say, "If management has better ideas as to what research projects we should be working on, they should be working in the laboratories. Actually, you know, we sometimes hire men to work on specific ideas, but after all, even those ideas are originated by technical people in the first place."

Dr. Dartell explained that many technical people started at the central research laboratory and then transferred to positions elsewhere in the company. He then proceeded to question Damon in an apparent effort to find out the areas of scientific work in which the latter was particularly interested. Damon said that he had done a lot of different things in the past and had been interested in all of them, particularly the work he had done in the development area. Dr. Dartell named several specific areas of work then going on in the materials department. Damon expressed interest in the first few mentioned but balked when Dr. Dartell got to the pilot plant area or to basic research. He finally expressed special interest in one specific problem, apparently because no one had done any work on it and there seemed to be a good chance that someone who did might come up with a valuable result very soon.

Dr. Dartell then brought up the question of salary. Damon said that he had not been used to discussing this question during his visits to various laboratories, because his experience had been that salaries were pretty well

standardized for new men anyway. Dr. Dartell's response to this was: "That's not true here. Salary here depends on the man and his ability to contribute in his field rather than whatever his position in the company may be. Many of our men here are paid more than their section heads. You don't have to be an administrator around here to get ahead.

"However, we don't have much spread in salaries in the beginning because we simply don't know as much as we would like to know about a new man until he's been with us for a while. Later on, when we can place a new man somewhere on the spectrum of talent in his field, we pay him whatever he should be paid for the contribution he is making. Once we have a man pretty well evaluated insofar as his talents are concerned, we then go out to other laboratories and find out what they pay their best people in any particular field.Then we usually go ahead and pay our men more than we find is being paid elsewhere. For instance, you take Gerald Schwartz. He is the top man in his section from a scientific standpoint, and he happens to be the section manager. But this is not necessarily so throughout the department."

Damon observed that this philosophy seemed to be slowly permeating into other companies that he had visited.

Dr. Dartell then asked Damon for his reaction on what he had seen in the two days he had been at the laboratory and whether he was interested in a job in the materials department. Damon said that he would like to think about it and would let Dr. Dartell know. At this point the interview ended.

A Discussion of Candidates

Later, on the same day as Dr. Dartell's interview with Damon, the observer was sitting in Gerald Schwartz's office with Dr. Dartell when Dartell decided he should call a meeting to talk about a number of the candidates who had been through the department in the past few days. He called his secretary and told her to set up a series of discussions on these candidates and then left the office. The observer remained talking to Dr. Schwartz, but about half an hour later Dr. Dartell's secretary came in and said that Dr. Dartell had sent her to tell the observer that a meeting was going on which would probably interest him and suggested he might come down to the conference room.

When the observer arrived at the conference room, he found a discussion in progress concerning one of the candidates. Dr. Dartell was seated at the head of the conference table with a stack of manila folders in front of him, and a number of other men were grouped around the table. The researcher noticed that there was only one section manager present. He had not been at the laboratory long enough to recognize any of the other men, but he later ascertained that they were all research associates.

The observer noticed that Dr. Dartell would announce the name of a

candidate who had apparently been interviewed individually by the men present, and would then ask each man in turn around the table to give his appraisal of the candidate. It seemed to the observer that Dr. Dartell was trying to establish (1) whether the man was regarded as generally competent technically and (2) if so, whether he would fit into the organization. When the observer entered the room, the discussion on the first candidate had just ended, and Dr. Dartell had asked Dr. Forrester, the section head involved, if he wanted to make a decision on this particular man or if he wanted Dr. Dartell to make the decision. Dr. Forrester said that he would make the decision if Dr. Dartell asked him to. Dr. Dartell said, "O.K. You go ahead and make the decision and I'll have the right of veto." [Laughter.] Dr. Forrester voted, "No," and Dr. Dartell said "O.K. Sorry, gentlemen, the decision is no." A number of men in the group then got up and left the room.

Dr. Dartell then named another candidate and said to the man to his left, "O.K., Jack, you start."

SCIENTIST No 1: I was very much unimpressed. He didn't seem to understand what we were doing, and I'm not even sure that he knew much about the work that he did for his PH.D. thesis.

SECTION HEAD: I thought he had a nice personality, but he was completely unaware of the scientific approach. His background is poor, and I would rate him below average.

SCIENTIST No. 2: I think he is below average technically. It may be that he has always worked on government contracts.

Other men around the table added to the negative comments on this particular man. After a little discussion, Dr. Dartell said, "You've heard the decision," and then said that was all for the Forrester group.

A new group of men came into the room. There were some questioning glances at the researcher. Dr. Dartell finally gave a few words of explanation about his presence in the room, introducing him simply as an "observer." The men sat down, and the meeting progressed as follows:

DR. DARTELL: Gerald Schwartz—Mr. Whalen. . . .

DR. SCHWARTZ: I say, hire him.

SCIENTIST No. 1: He seems to have a good personality, and he seems capable to me.

SCIENTIST No. 2: I think he is a very intelligent fellow. He has a good background in some of the areas that we need to work in.

SCIENTIST No. 3: I'll go along with that. He made some pretty pointed comments on the work I presented to him. He said "Ah" in the right places.

SCIENTIST No. 4: I thought he was a pleasant fellow and had a good background for a physical chemist. [*Laughter.*]

SCIENTIST NO. 5: It seemed to me he had a wonderful combination of talents.

DR. DARTELL: We would like to hire him then? [*There seemed to be general agreement that this would be a good man to have in the laboratory.*] O.K. Let's talk about Williams then. [*There was a good deal of interim talk about a man by the name of Winkler before they talked about Williams.*] O. K. Charlie, what is your impression of Winkler?

SCIENTIST NO. 1: I have a feeling that he may be brilliant, but there is no way that I can uphold that feeling. From the standpoint of his personality he seems irrational and immature, and he doesn't seem to have any confidence in himself. Technically, I think he is very efficient.

DR. SCHWARTZ: I'm going to have to run, so I'd like to give my opinion on Winkler. Technically he seems quite competent; otherwise I didn't get much of an impression of him. [*Dr. Schwartz left the room.*]

SCIENTIST NO. 2: He was able to follow my descriptions of the work we were doing and anticipate the next step. Socially, however, he didn't seem to be very good.

SCIENTIST NO. 3: I saw him only socially, and I can only describe him as being rather irrational. It may be that he can carry out routine work.

SCIENTIST NO. 4: Paul is right. Technically he's good, but he has a view of the world that I consider dangerous. [*Laughter.*] I think he is a paranoid myself. He seems to have a superman complex. He asked me, for instance, if we prepared a script for the luncheon at which we interviewed him—that we couldn't possibly talk like that all the time. I have some reservations except, darn it, he seems to have a quick mind.

SCIENTIST NO. 5: He's no more mature than when I knew him five years ago. He just doesn't believe anything in the world is as it seems to be, and he is very self-deprecating, but on the balance I just don't know.

DR. DARTELL: [*Shouts.*] Harold—Mr. Winkler. . . .

DR. FORRESTER: I really don't know.

DR. DARTELL: Last night we had a couple of drinks, and he said point blank that the whole day was arranged to fool him. [*Somebody in the group asked, "Did he say that belligerently, querulously, or how?"*] No, this is his considered opinion. Also, he is not sure whether he wants to do scientific work. [*Dr. Dartell read a number of comments from the man's professors, all of which extolled his technical ability and some of which said he had an excellent personality, although some comments expressed doubts about this.*] He's not very different from Al Grass [*one of the men at the table*] when he was interviewed. [*Grass laughed and said that he thought Winkler was very confused when he boarded the plane to go home after the interviews.*]

SCIENTIST NO. 6: I saw him the whole day at Harvard. I think he would be an asset to the laboratory. As for his perversity, I think he'll get over it as Al did. [*Loud laughter.*]

DR. DARTELL: O.K., boys, what should we do? Yes or no?

Two men ducked Dr. Dartell's question when asked specifically whether to hire the man or not. Dr. Dartell tried to pin them down, asking for a yes or no answer. The second man said that that wasn't fair and wondered how badly they needed new people. Dr. Dartell said the need was not critical. The second man finally said yes and another man also said yes when asked his opinion. Scientist No. 4 voted no. Dr. Dartell told him not to base his opinion on the luncheon, but the scientist replied that he felt he had to because Winkler didn't believe the men at the meeting were saying what they meant. Dr. Dartell observed that the luncheon conversation seemed to him to show up a lack of scientific knowledge on the part of the man concerned as well as an inability to think in abstractions.

Al Grass voted no, and said that he didn't think the man was brilliant enough to hire considering his personality. There were several more no's around the table, and then Dr. Schwartz, who had come back into the room, said yes. Dr. Dartell argued that on the basis of the candidate's lack of understanding of either life or science, particularly the former, he wasn't sure that this was the kind of man they wanted. Scientist No. 1 said, "I have a hunch that the guy is brilliant but extremely confused, and it may be very worthwhile to take the gamble."

A new man walked into the room. Dr. Dartell told him that they were talking about Winkler and asked him for his reaction to Winkler on the basis of his interview with the man at Harvard. The man said he thought a lot of Winkler. Dr. Dartell looked rather puzzled, and said that he couldn't understand how these men could see Winkler as being so good. Someone asked about the professors' reports on Winkler, and Dr. Dartell read them again. Once again they seemed to be almost entirely favorable. One of the scientists at the table said, "The question is, can he live among us and do very good work? I'm sure he isn't going to hurt anyone, but his personality is so extreme that it may get in the way of his scientific work."

After further discussion, Dr. Dartell summed up by saying that it was a very difficult problem and that he was as confused as everyone else. It appeared to him that the man was O.K. technically, but at lunch and in the evening he had never seen anyone so determined to be irrational. He then said that the group had taken enough time on this particular man and that Dr. Schwartz and he could talk it over with some of the other men and try to come to a decision. He then proposed to discuss the candidate Williams who had been tabled before. The discussion on Williams seemed to focus on his difficulty in talking about his work. The final decision was not to hire him.

Later in the day Dr. Dartell told the observer that he had decided to hire Winkler on the basis that most of the men seemed to believe that he had an excellent potential technically even though he was a crackpot. "Generally speaking," he said, "the technical ability or potential is the prime factor in deciding whether or not to hire someone. Personality

helps, but it is not essential. For instance, we have had a schizophrenic in the laboratory for some time who has tried to commit suicide three times and who has just returned to the laboratory after his second series of shock treatments." The observer expressed some surprise that they had retained this man on their staff. Dr. Dartell said, "Our criterion is that if a man is not likely to hurt anyone else or the work in the laboratory, we keep him on. Some such men are problems, but you have to live with them and handle these problems if you are going to get the best technical men available." (Several days afterward, as the observer later learned, the decision to hire Winkler was reversed.)

Further Observations on Recruiting

During the weeks that the observer spent at the central research laboratory, the subject of recruiting came up several times as he talked with a number of men in the materials department. He recalled especially two such discussions. The first of these was a conversation with Harry Rice, the personnel representative on Dr. Dartell's staff. Rice had this to say about recruiting: "Probably we talk more about a man's psychological makeup during our recruiting effort than most places do. The question is, do we balance this properly against a man's technical ability? How much do you want to put into the effort to help a man get along with other people? Quite often when you get a couple of drinks in a recruit after we leave the laboratory in the evening you learn a good deal about his true character, and this has turned out to be pretty fortunate a couple of times. I guess we are pretty peculiar here. None of us are experienced interviewers. I've gotten conscience-stricken occasionally. I'm trying to read up on it, but I haven't found very much that makes sense in the literature. In fact some of it is fantastic. I have the same trouble with books on organization and communication. Maybe I just don't speak the language. Of course, Carl agrees with me on these things, and I guess that helps."

The second discussion of recruiting which was particularly interesting to the researcher occurred during a conversation with Alan Wirtheimer, one of the senior scientists in the materials department. That portion of the conversation which is pertinent went as follows:

WIRTHEIMER: To get a real picture of this department, you have to realize that the man in charge is a controversial figure in the industry. People coming in from elsewhere often ask me about Dr. Dartell. They have heard conflicting reports about him.

OBSERVER: Are you saying that the most important thing to people coming into the laboratory is the man in charge? Of course other things are important, too, like the working conditions and the degree of freedom for work and so on, but it seems to me that you have been saying that the primary consideration is the man who runs the place.

WIRTHEIMER: Well, you were present once when we reviewed some candidates. My section manager and I were talking one day, and we agreed that no man should be hired unless Carl liked him. Carl has a fierce loyalty to his staff, and even if everyone else likes a man, if he doesn't, that man doesn't belong here. Dartell goes out and tells everyone this is the best staff there is. He really believes this, and it is important that he does. He couldn't believe this if the men did not get along with him.

OBSERVER: No man would come here who didn't get along with Dartell?

WIRTHEIMER: I know of no case since Dartell has been department head. I understand there was quite a conflict between Dartell and some of the men who had worked under the previous department head, and a number of them left, but in the two or three years he has been department manager, no one has come with the organization who didn't get along with him personally.

Chapter 32 (Concluded)

CONTINENTAL ALUMINUM (C)*

DEVELOPMENT OF RESEARCH MANAGERS

As HE watched Dr. Dartell interact with members of his technical staff, the observer came to believe that the department manager placed major emphasis on administrative problems related to the training and development of his five section managers. Several times during his stay at the central research laboratory, the observer had been asked by Dr. Dartell if he would care to comment on possible methods of selecting and developing section managers. He also knew that, as one means of increasing his own understanding of the problems involved, Dartell had asked Harry Rice to devote considerable thought to the development of a supervisory personnel.

Dartell had told the observer that he himself had learned most of what he knew about administration by sitting at the feet of a former superior, whom he characterized as "the best administrator I have ever known." This man had formed the habit of inviting his young subordinate to come to his home for a cocktail four or five times a week, and over martinis the two men would discuss the events of the day. Dartell believed that these sessions had been by far the best training he had ever received.

It seemed to the observer that Dartell had used similar methods in passing on his administrative philosophy and techniques to his section managers. With the possible exception of Bill Christopher, the young acting manager of one of the fundamental sections, and Harold Forrester, who had been made section manager of the development section less than a year before, the observer believed that a very close personal relationship existed between Dartell and the individual section managers.

At one time or another the observer talked at length with each of Dartell's immediate subordinates. He was particularly interested in statements made by Alan Pike and Bill Christopher as well as Dartell's plans for these two men and the way these plans related to the further development of Harold Forrester, manager of the development section.

Interview with Alan Pike

Dr. Alan Pike had been section manager of one of the fundamental sections for five years, but had had a change in his assignment a few months

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before the researcher's visit. At that time, he had been asked by Dr. Dartell to become a member of a team of professionals from various divisions of CAC who were to spend six months studying the technical activities of the company in relation to cost and efficiency.

During the course of his visit at the central research laboratory, the observer spent a morning with Pike. Excerpts from notes of the interview follow:

OBSERVER: I am primarily interested in you and what you're doing. I'd like to leave the conversation as open as possible and let you pick it up anywhere that suits you.

PIKE: Do you mean the work that I'm doing now or my regular work?

OBSERVER: Both.

PIKE: Well, right now I am working with a group of 12 men who have been recruited from the company as a whole. We are studying the technical activities of CAC. It would seem unlikely that the company is organized to make the best use of their technical people. Therefore, they want to study the question, and I am one of the group that is doing this work.

OBSERVER: That sounds interesting. How are you going about it?

PIKE: We're not leaving anything out. We have split up, really, after acting as a group in the early part of our project, into 12 individuals—each of whom is tackling part of the problem. The thing I am working on is to see if it is possible to decide what the optimum amount of technical work is that can be done in any one company component. I'm interested in trying to find out whether we can do more or less technical work in various parts of the organization. Obviously, if you can increase the technical work and thus optimize the profit potential of an organizational component, this would be fine, but the question is—how do you know that this would be so?

OBSERVER: You're not concentrating on just the scientific people here at the laboratory then?

PIKE: No, I'm working on the problem of all the technical people in the company. This laboratory, of course, is important to the total study and is particularly close to me, because I work here. It's taking a fair amount of the total effort of the group to study the research laboratory.

OBSERVER: What are your feelings about what you are doing?

PIKE: Well, you know there are a number of laboratories in the company. Most of them belong to an operating component. This one belongs to the corporation at the top level. I believe the primary thing this laboratory should do is basic research. Past history has shown that new business has come into being as a result of fundamental research, and therefore it is a good investment. But of course this is not what you tell people here in the lab. CAC is motivated by the need to make money, but the men want to rise in the scientific world. You have to give them

stimulating surroundings and then give them a free hand to operate in these surroundings as they want to. Of course you acquaint them in a general way, and not too often, with CAC problems, so that they will have them in the back of their minds. But not so much that they will worry about them. And then you turn them loose.

OBSERVER: Do you find any opposition to this viewpoint of the function of this laboratory?

PIKE: No, this has been the viewpoint of CAC management for a long time; it has soaked into the company for a long time.

OBSERVER: Then as far as this laboratory is concerned, the study will probably turn up nothing new?

PIKE: That may be so. In fact there are many things we certainly would not want to change. It's then up to management to take any action they believe desirable. Since they have taken 12 men off their jobs for six months, I guess you can assume that they will probably act on some of our recommendations.

OBSERVER: Well, let's get back to your old status here in the laboratory.

PIKE: Well, maybe I'd better go back a bit. I thought in my youth that I wanted to be a physicist. As a result I took a lot of math when I was going through college. I then discovered that math was not very good training for graduate school. Then I had an opportunity at Battelle Institute. I worked there in the metallurgical field for a year and found it interesting. Subsequently I went to graduate school at M.I.T. in the mechanical engineering field. While I was at M.I.T. I met Dartell. I remember that I heckled him at a colloquium that he was leading. He asked me how I'd like to work as a metallurgist somewhere, and it later turned out that the somewhere was going to be CAC. I started learning metallurgy, chemistry, and so forth when I came here. This was six or seven years ago. When the former manager retired, Carl became manager of the department and split the staff into sections. He asked me to be a section manager. Not knowing what was involved, I said yes. After I had been doing the work for a couple of years, I found out some of the things that this decision meant to me.

OBSERVER: For instance?

PIKE: Well, your viewpoint changes. You have to get most of your satisfaction out of helping others. Insofar as your own specialty is concerned, you don't accomplish so much. I've done research in my field about half time since I've been section manager. Whether I'd be twice as effective if I were doing research full time, I don't know. If you do, of course, you are more likely to make a name for yourself technically. On the other hand, you can do pretty well and still be a supervisor. When I look around at the physicists with national or international reputations, I find a lot of them in supervisory jobs. This job I have seems fairly easy, as supervisory jobs go. It may be because I have the same training as the people in the section, and these men are largely self-starters. I would talk to

them anyway, whether I was a supervisor or not. In fact, I'd probably be rougher on them if I wasn't a supervisor. The main thing that takes nervous energy is when a man is in the wrong job for him and should be working elsewhere. Then you have to do something. I used to feel you shouldn't mess with other people's lives; that you should let things like that go; but I have found that if you do, things usually end up worse than they were; so I just charge in and do the best I can.

OBSERVER: I gather you do not see this as one of the pleasanter aspects of your job.

PIKE: That's the way I looked on it at first, but I found that it doesn't happen very often and that if you succeed in straightening a man out and put it to him the right way, it's really very satisfactory.

OBSERVER: From what you have said so far, I think you are implying that you sort of stumbled into this job.

PIKE: Carl said I'd never know if I wanted to be a supervisor or not.

OBSERVER: What happens in the future?

PIKE: I would like to continue in research and research management. I've decided that this laboratory is the one place in the world where I really want to work; the result is, when I get offered jobs elsewhere, I turn them down. I believe the people here are wonderful, and I certainly have all the freedom I could ask for. There's a chance that when I get older I may lose the research spark. If so, I will get more into the business end. I have no particular spot pinpointed for myself 20 years from now. I'm just happy to be doing a good job at any of several activities.

OBSERVER: As I have been traveling around research laboratories, I hear people constantly expressing what I regard as a false dichotomy between research and administration. I gather that this particular problem doesn't worry you.

PIKE: Specialists sometimes think of all the good men who might have continued as valuable contributors in their special fields but who have been lost to administration. Well, they are lost, but they also get a lot of satisfaction, I believe. You can take either extreme, or wander down the middle doing both research and administration. Most of my life I thought that I wanted to be a specialist, but I have a lot of interest in administration. I guess now that I won't wander very far down either branch but will try to stay half and half for some time.

OBSERVER: I believe that the opportunity to do this is rather unusual. We don't often find, in other laboratories, men in the supervisory group doing much research.

PIKE: This kind of job almost requires you to do research. You have to know if the men in the section are doing sound work. You have to criticize and lead them. There's a better chance to do this if you yourself are doing work that commands respect in the same field as your men.

OBSERVER: This having to do work that commands respect—I suppose

it's a silly question, but I've heard a number of men say this and I wonder why this is so necessary.

PIKE: Well, I've never heard the question asked before. You know a lot of the fun of being a chemist or any other scientific specialty is going around to your colleagues and saying, "Look what I did." If a man can't tell his supervisor about his work, that's not so good. The supervisor ought to be able to recognize a step forward. This means he has to be capable of good work himself. One way to prove that you are capable is to do some work. It's hard to stand on a reputation made years ago as far as new people coming into the organization are concerned unless your reputation is a real healthy one.

OBSERVER: A technical supervisor has to prove he is capable of doing better work than his men?

PIKE: That puts it a little stronger than I meant. It's helpful to him. There's a principle I've heard that you don't want to compete with your people, and I believe that is so. But you're not competing if you pick out some segment of the work which is your own size and work on it. This is particularly true of me and Schwartz. We do more of this sort of work than the other section managers. It's less true of Lee and still less true of Forrester.

OBSERVER: I've heard that before, too—that supervisors in fundamental research can do more research on their own than supervisors occupied in the development end of the spectrum. I wonder why that is so.

PIKE: Well, take Dartell, for instance. He is an extreme case. He does individual work but of a different kind. I do the kind of work that my men do. He does work that is different from that of other people. It's more appropriate that Schwartz and I do work like our men.

OBSERVER: The reason I'm asking questions in this area is that this is the first place that I've visited where it is so easy for you to go down the middle road and spend part of your time on research and part on administration.

PIKE: I'm not sure if Forrester can do this. In his job he could devote full time to supervision and be very satisfied. It's less likely that a fellow could do that in my area.

OBSERVER: It's almost as though you were doing your individual research work to help you keep out of your men's hair.

PIKE: Sure, I'm supposed to lead them, and this to me means doing the kind of work that would be desirable for them to do—in other words, be an example to them. The program of the section, after all, is not conceived by me, but by the men who do the work. When I get an idea for a program I have to sell it. Sometimes I can and sometimes I can't. For instance, I've been after Harlow Graff for six months to try to get him to make certain measurements. One of these days he may do this work. In the meantime, he's doing some of the best work in another field that has been done.

One reason that it's possible to take the middle road is that, when you get into applied work, business decisions enter in more heavily. I imagine that it would take all your time to supervise your section under those circumstances. But in basic research you've made your decisions when you hire a man. We are sheltered from business decisions by the divisional laboratories. We sometimes hear of such things, but we don't have to do anything about marketing decisions, for instance. One thing that's important is that CAC is so diversified. This means that it's hard for a man not to work on something which is of interest to the company.

OBSERVER: Is the work you are doing with the group of 12 in any way connected with the job evaluation program?

PIKE: No, it's not. My job primarily is to measure the contribution of various functions in some sort of quantitative terms. Where should money be invested in technical people so that the company gets the greatest return for their investment? For instance, should this laboratory be bigger or smaller and if either, how much?

OBSERVER: Can you answer that question?

PIKE: I think I know how to go about it. Give me five years, and I probably could answer it.

OBSERVER: Is this operations research?

PIKE: I suspect it is. Or you could look on it as a good system of cost accounting.

OBSERVER: I'm particularly interested in this question because everywhere I've gone I've heard this need expressed to measure the contribution of the research organization to the company as a whole.

PIKE: The president is the one in this company who's responsible for that. He wants to measure the contribution of every component.

OBSERVER: Suppose you succeed in doing this—what are the implications to basic research? Would this make the supervisory job harder?

PIKE: No, I just want to help the man who has to decide how large a research organization should be by giving him better information than he now has. The judgments have to be made now, but on the basis of intuition or something of that sort. For instance, the reason that the materials department is growing is because of Dartell.

OBSERVER: What do you mean by that?

PIKE: Well, he stimulates people so that they come up with ideas that are worth putting money into. When you have good people in an organization, you want to spend a lot of money there. If you have poor people, you cut down. This means you have to judge how good your people are.

OBSERVER: This decision is made for you to a large extent by the scientific community?

PIKE: Yes, but somebody has to decide how they stack up to their colleagues here. Also, how far do you go in paying for top men? Sooner or later you get to the point where you don't get any return for a higher

paid man. You can see that I've taken over my scientific training into my new job. I'm trying to analyze and measure.

OBSERVER: I suppose it's the attitude behind the analyzing and measuring. In some companies, for instance, when the cost report hits the manager's desk, everybody in the organization knows it because it puts pressure on him which he transmits to his people.

PIKE: But, of course, in research results aren't apparent for five to ten years; so it's a long time before you can look back on what has been done and say that the lab should have been bigger or smaller. Also, it would not do any harm, as far as the rest of the company is concerned, if it were known—instead of just suspected—that this research operation was economically sound. There's some misunderstanding about this, especially since the company is hiring new people all the time. They think they should get something out of the research laboratory. Although they are being assessed for it, it is not run for them but rather for the company as a whole. If they knew it was making money, it wouldn't bother them so much.

OBSERVER: I am interested in the fact that the need is being felt to measure and analyze, especially since you've apparently been getting along just fine through the years.

PIKE: Not me. I can't understand anything I can't measure. That's probably why I flunked economics. There was only one equation in a great big thick book, and that one I didn't believe. I guess being able to make sound judgments with a minimum of measuring is how men get to the top in administration, but I suspect they would like to be able to measure more.

OBSERVER: You are uncomfortable having to make decisions based on something you can't measure?

PIKE: You're right. I have to do it all the time, of course, but I don't like it. I'd like to be able to predict what would happen. I think this is what the fellows who are good managers actually do, and their best judgment is probably a lot better than one might expect.

Interview with William Christopher

The day after he talked with Alan Pike, the observer interviewed Dr. William Christopher, the young scientist Dr. Dartell had appointed to be acting section manager, while Pike was on special assignment.

OBSERVER: I am particularly interested in you because you have taken on this supervisory responsibility on a temporary basis. I'd like to hear something about your thoughts on that situation, but why don't you pick up your story wherever you want to?

CHRISTOPHER: I came here about three years ago in August. Like most people I really didn't have much of an idea about what I wanted to do

when I got here, but then I got interested in an area of technical work close to what I had been interested in at school and have been working on it ever since with Alan Pike and Granville. I'm not at all sure about the underlying reasons why I ended up in this job this spring. In any case I'm happy about it because I've found out quite a bit that I had wanted to know. Of course, I'm plagued with the defect of liking to talk to a lot of people; so I enjoy this job. As you've gathered, we've had a heavy recruiting season, and I've had a lot of talking to do. Also, I've participated in planning the new wing. Other parts of my job are things like planning the section's activities, encouraging people to do things I think they're interested in, and looking into the service functions.

The way I view the job of section manager is not so much to assume authority as to make sure the men in the section don't have their hands tied by being loaded with administrative work, and therefore I try to do all the paper work myself. There are some areas where one has to do a bit of independent thinking. For instance, at what point or on what basis do you decide that a man needs a laboratory assistant? At one point I tried to reduce this problem to a series of analytical criteria, but this just doesn't work. Maybe the best way would be to take all the laboratory assistants away and then give them back to the various men in the order of real need.

OBSERVER: Is that a recurring problem?

CHRISTOPHER: It's a constant problem to which the materials department has given considerable thought. Of course, some people wouldn't be caught dead with a lab assistant, but others want five or six. One thing we're sure of is that the more laboratory assistants you have, the more supervision you have to give them and, therefore, the less time you can spend on doing work yourself. Consequently, there is a maximum desirable number.

The main purpose of the section manager is to try to encourage the work of the people, to make sure that the others know what they are doing so that they can cross-encourage each other. Then there is the additional problem of trying to determine what areas are critical, what problems we are not tackling that we should be, and then you have to try to encourage people to work on these critical problems. Essentially, in doing this you collect data from the men who are in the best position to know what the critical problems are, the men who are doing the work. I feel strongly that this job has nothing in it that gives one the authority to tell someone to do things. Of course, we have to be sure that the men are doing good work.

OBSERVER: Someone has to judge whether the work they are doing is good work?

CHRISTOPHER: What we aim at is to have people in the sections who are the top men in their research field as judged by the society in which they operate. By this I mean a scientific organization, like the [American

Chemical Society] or the American Physical Society. Doing it this way, we have full knowledge of all the basic research being done in our field. This implies that one of the jobs of the section manager is to encourage people to be the best in their field, to write papers and talk to top people in their field, and to get people to come here and talk to our people so as to keep them informed and stimulated. You have to foster a relationship between the men here and the community of scientists to which they belong. We should be informed about, and be able to translate, all good scientific work in our field in the world. It's not only important that our men be good people as far as their relationship to the company is concerned but also in regard to their relationship to the scientific community.

OBSERVER: So in essence you see the job of supervising research as being almost a service function.

CHRISTOPHER: For the most part. Obviously you have some responsibilities for making decisions concerning people.

OBSERVER: For instance?

CHRISTOPHER: Well, I mentioned the lab assistant problem for one. Basically, the decision on what the critical areas really are comes after the men tell you what they would like to do. Another thing I would like to call a service is steering the men to the work they do best. I think one can judge where a man's basic interests and talents lie. This doesn't mean that you dictate to a man. If he wants to drift from basic to applied work, you should not hinder this. By steering, I mean not encouraging things you think he is not really interested in or things you believe are not really significant to him.

OBSERVER: That is an interesting distinction.

CHRISTOPHER: I have some reasonably strong ideas about what the section manager is and should do. He does not have to be the strongest technical man in the section. The company should not reward the best technical man by putting him in a position where he can't do any more scientific work. I also feel strongly that we should not have scientific group heads. This would lead to placing a ceiling on technical performance because people would then feel they had to displace these men to grow. They should be able to grow without reference to the organizational system.

I know I'm not the most technically competent man in the section. I just like to do this kind of work. There are men in the section who are better technically than I am who would not be caught dead doing what I am doing. But the section manager cannot be so disassociated from research that he does none himself. He has to be able to understand the problems and how people think and work so he can do the encouraging function. So the section manager must continue to do some research. This is not true of the next step, the department manager, because here a man is dealing with groups of people.

OBSERVER: Referring to your last statement, are you implying that the fewer levels of supervision you have in a research organization the better off you are?

CHRISTOPHER: Yes, but it's also important that the section manager encourage people in only one broad area. It's hard for one supervisor to encourage and reward men in both research and applied work. He could not put the same sort of weight on their problems from their point of view. One problem for me is a man I have who is engaged in designing devices for testing, which is applied work, even though it's being done in this section. I try to place equal weight on his work, but I think that he feels I am primarily interested in basic research and he may feel misplaced.

OBSERVER: You are afraid he doesn't think you see the significance of this work?

CHRISTOPHER: That's right. There's always a difference between how the section manager sees his job and how his men see it. It's not really a serious problem, and I don't want to overemphasize it, but many men look at the section manager as a "technical decider" who makes decisions about their work. From my viewpoint, I'm an "encourager."

OBSERVER: You have to wrestle with what they expect of you rather than what you do?

CHRISTOPHER: It's not so much what they expect. I don't like the word "expect." But take, for instance, the problem of analyzing people's report material, how much they turn out, and so forth. From my standpoint this is valuable as one approach to evaluating people. From their point of view, if I knew someone was going over all the reports turned out this year, I would suspect that this would imply an accent on reports that would not be wise. In short, it's not what you do, but what you might be inclined or have the authority to do that's important to some people. It's possible for you to exert authority and to place emphasis where the man doesn't think it should be placed. It means you have to be more responsible in operating the same way you always did. When I was in the lab, I would not hesitate to tell a man he had a hole in his head. I still do, but I have to do it differently. A man's feelings about what you say are more critical when you're in a supervisory job. The section manager has to recognize what the structure really is; he doesn't make it. The men do, and they know who is doing the significant work.

OBSERVER: Are you saying that you've been set apart from the main group of the men now that you are in this job?

CHRISTOPHER: Well, I won't go along entirely with that. When Alan was here none of us had any hesitancy in talking pretty frankly. In this sense, he was not set apart. One simply realized he had this responsibility. How much you are set apart depends mainly on your behavior.

OBSERVER: Of course, you are responsible for evaluating people for salary purposes. Doesn't this set you apart?

CHRISTOPHER: No, because if the men made lists of those who were doing significant work, they would be the same as mine. Just so long as you let them know that you recognize the status of the various men and that your judgment is fair, no one will worry. There is some separation because my task is different, but the real separation or closeness depends on my behavior. Basically, anyone else in the section could to this work. I'm taking the burden of doing it off their shoulders. They would look upon it as hindering their work, but I like to do it.

OBSERVER: On evaluation, you are not judging the men in your section on the basis of your own criteria but according to the evaluation of a man made by his colleagues?

CHRISTOPHER: Yes. It is on the basis of the standing of the man as seen by his peers. A person could be here and make no individual contribution, but he could stand high because of his encouragement and help to others. That's why I can't judge solely on the basis of patents or papers. I have to look at the group as a whole because, after all, we are not all in separate little boxes.

One problem that might arise is—suppose you have two men. One cannot think up new ideas, but his performance as an experimentalist is excellent. Another man gets ideas that he passes to other people. I have then to decide which is making the best contribution. I can't make this decision if I'm standing outside the society, the little group. You have to put yourself in the group and try to measure a man's impact on the group. You then reach the decision the group has already reached. The section manager must be imbedded in the group. He should be able to find out a man's wildest ideas as well as his well-thought-out ones. Then he will know who the man should interact with so as to get the best discussion of his ideas. You can't sit in an office and consider only their well-thought ideas if you are to understand the men.

OBSERVER: Where did this philosophy of management come from? Where did you get these ideas?

CHRISTOPHER: I had this philosophy when I came here. I recognized its existence here, and I want to foster it. Basically I guess I got it at the university. The whole concept of the researcher as an individual, I mean. It's the only way you can do good research in an organization. I guess there's no one place I got this philosophy, it just. . . .

OBSERVER: It just growed?

CHRISTOPHER: [*Laughs*] Wes. It's just axiomatic that for intellectual freedom a man must be allowed to grow as an individual. Of course you have to have in mind the interests of the company. You can't encourage men to do work in an area of no interest to the company. But that's no problem, because we define the framework by hiring men who are interested in our basic fields. This does not imply that we have to limit ourselves to the present problems of the company insofar as basic research is concerned.

OBSERVER: How about the relationships between your group and other groups?

CHRISTOPHER: Obviously, the interaction between people, in order to get the job done, has to be done on an individual basis. It's ridiculous to pass information through the section manager. It should go from man to man. The section manager's job is to see to it that this is happening. Men in the middle group [*Christopher had labeled the three groups in the department "fundamental-middle-applied"*] have the responsibility of passing information back and forth between fundamental and applied, and you need strong men in this group to handle this process.

At the present we don't have strong men in all cases, and so we have to use expedients. For instance, we have a strong man in this group who is devoting part of his efforts to a field that really belongs in the middle section. As an expedient he is performing this function, because no one in the middle group can do it as well. It's my feeling that this is not a good thing. His job should be in the area of trying to *understand* the field, and the function that he is performing dilutes this effort. That is why people in the middle area are best suited for this communications job. They have some basic understanding and can talk to the fundamental people. They also have knowledge of the relationship between particular structures and particular properties and can talk to the applied people.

As far as the relationships with the rest of the company are concerned, this is the job of the applied people. But all of this has to be at the level of the technical people. You can't do this by the administrative superstructure method. That's a relationship I don't quite understand. For instance, we have section seminars where people are supposed to talk about the work they are now doing, rather than work they have done, so people can make suggestions and give them some ideas that might help them. But you get fewer suggestions in this organized way than later, after the meeting, when individuals get together. So just the introduction of a slight bit of formality inhibits ideas flowing between people.

OBSERVER: How about your future? Will it bother you when Alan comes back and you have to go back down to the bench?

CHRISTOPHER: Obviously I've been thinking about that. But I object to the idea that I'll be going "back down." I'm just going to another type of work. On the whole this has been a good experience for the last six months. I've been able to see things that I haven't had a chance to see before. Candidly, I enjoy this sort of work. I would be happy to continue it. People get interested, I guess, in almost any environment that they find themselves in. I don't think I'll feel badly about going back to research work, but I like to worry about other people's problems, and I would like to continue doing this sort of thing. This activity won't be completely stopped when Alan comes back, of course. I'm just doing more of it than I would normally do.

This job requires a reasonable percentage of your time; so your

technical work is curtailed. You have to have the attitude that it isn't so much that you do work as that work gets done. You do have to have some time to do work yourself, but the important thing is to think about the work being done. Of course, I'll have to adjust to the purely technical work again, just as I had to adjust to this work. I myself feel that I could advance further doing this kind of work than by doing technical work.

OBSERVER: The probability is that this isn't your last chance to do this kind of work?

CHRISTOPHER: It may sound wishy-washy, but I don't think my potential in either area is so overwhelming that I should decide now which way I want to go. Part of the reason why I don't feel the need to do this is my strong belief in this laboratory and particularly in the philosophies of this department. This place has something that I want to be a part of and contribute to any way I can.

Dartell's Plans for Pike and Christopher

The interviews with Alan Pike and Bill Christopher were particularly interesting to the observer in relation to subsequent information that he obtained concerning Dr. Dartell's plans for the two men. The observer recalled particularly one conversation that started when Dr. Dartell asked him whether he thought Pike or Christopher should manage the fundamental section. The observer did not answer the question directly and Dartell continued:

DARTELL: You know, five years ago, when I made Alan Pike head of that section, I was sure I could teach him how to handle people. Now I realize that he is essentially a problem solver who is uncomfortable with nonrational phenomena. I've purposely put myself in the position where I can decide either way on this thing. If Bill does not work out, I can return Alan to the job. If he does, I can transfer Alan elsewhere. I'm also setting things up so I can make the transition gradual if I decide Bill is the man for the job—which frankly I have just about decided.

I purposely set up Alan for this special assignment—in fact, I had to work pretty hard on Dr. Zanke to get him to pull Alan off his regular job. I did it for two reasons: first, to give Pike six months to think about his future and to give him a chance at the same time to work on some different kinds of problems. Secondly, and at the same time, this has given me a chance to give Bill supervisory experience so I could see how he handled himself in case I decided to transfer Alan to a different kind of job. I have a job in mind for Alan that I think is a good one, but I'm not sure what Alan will think about it. He may be unhappy about it—not because the job doesn't suit him, but because he would have to accept the emotional blow of admitting that he could not solve the problem of learning how to handle people.

It's funny, you know, the only time Alan really faced up to that problem was when he knew the study group job was coming up. I think Bill is much better than Alan on things of that sort, although he is not as strong technically as Alan. What I'd like to do is to give the situation a little more time to jell by assigning Alan to another special job when he finishes his present assignment. What I'm thinking of doing is asking him to go into Harold Forrester's section to help Harold work out the solution to a number of problems in connection with the move into the new building. In fact, while I think of it, I haven't mentioned this to Harold yet—I think I'll ask him to come in and sound him out on the idea.

Dr. Dartell promptly picked up his telephone and asked Dr. Forrester to step into his office for a moment. When Dr. Forrester appeared, he told him what he was planning to do. The researcher believed that Forrester evidenced considerable surprise at Dr. Dartell's proposal, but he said that he would be delighted to have Dr. Pike's assistance. He said he had plenty of problems that Pike could certainly help him with if Pike were agreeable to the proposal:

When Forrester left the room, Dartell continued to talk about the proposed change.

DARTELL: I think it will take maybe two years to solidify this change. In the meantime, Christopher will remain acting section manager.

OBSERVER: Won't that put some pressure on Christopher? The uncertainty of his position, I mean.

DARTELL: I don't believe so. I'll make it a point to give Bill good raises in salary and plenty of pats on the back. If I don't tell him my plans, I can retain the alternative of returning Pike to the job. You know, some months ago I talked to Alan and asked him to make a list of things he did every day that he liked doing and of those he disliked doing. It was clear to both of us, when he completed the list, that Alan liked problem-solving and disliked working with people. At that time I suggested that he consider some other kind of job, and I guess he has been thinking along those lines. I really feel badly that I couldn't teach that guy how to handle people. Five years ago, I would have bet anything I was going to be able to do it.

Several days later, the observer was talking with Harold Forrester when the subject of the proposed assignment of Pike to his section came up. Forrester had told the observer that, since he had been section manager of the development section for less than a year, he had not had time to prepare himself adequately to handle all of the problems he found confronting him. He continued:

FORRESTER: It would have been nice, for instance, if I'd had two or three years on the job before I had to take on the responsibility of the new

building. That, however, is Carl's worry. If he feels he has to call in Alan Pike to help me, I have to accommodate to that.

OBSERVER: Let me pick you up on that. What do you mean? I was there when Carl told you about Pike's new assignment. Are you seeing something in this I don't see?

FORRESTER: Well, in my opinion, Alan Pike is one of the most brilliant men on the staff. So Carl is asking Alan to come in and help me. I can accommodate to this arrangement because this building is an exceedingly important thing to the company. Ordinarily, I suppose this would worry me. After all, this is a very unusual proposition that only a fellow like Carl would attempt. When I said I was surprised the other day, I really was. One thing about it, I have worked with Alan before and he is a very easy, pleasant person to work with.

OBSERVER: It is an unusual thing to take a man of Alan's stature and slip him in under another section manager?

FORRESTER: Yes, but if I were Dartell, Alan Pike is the man I would select to improve operations in this section.

OBSERVER: It's unusual—yet you feel you can live with it?

FORRESTER: I know Alan. It could be anyone else, and I wouldn't be able to live with it. Alan, however, is a very mature, competent individual.

OBSERVER: Whereas another man would create a problem for you, but being Alan there is no problem?

FORRESTER: [*Nods.*] I can see where other people in my section will wonder what's going on, but that's something I can handle when it comes up.

OBSERVER: Do you feel this is a good move as far as the organization is concerned?

FORRESTER: On the basis of what I now know I think it is. Regardless of what happens, I can certainly use Alan's help.

Chapter 33

AVCO-EVERETT RESEARCH LABORATORY (A)*

Introduction

THE AVCO-EVERETT RESEARCH LABORATORY (AERL) was, in 1963, a moderately large (706 employees) industrial research laboratory doing advanced work on the military and civilian applications of high-temperature gas dynamics. This research included work on ICBM defense, magneto-hydrodynamic generation of electricity, plasma propulsion for space vehicles, and satellite reentry phenomena. In all these areas, and especially in the general area of high-temperature gas dynamics, AERL had established a reputation for preeminence, a reputation which was shared by several individual scientists working at the laboratory.

This case is one of a series which will look at AERL as a social system. Within this context, the cases will be almost exclusively concerned with the behavior of people at AERL and with some of the factors underlying this behavior. The factors examined essentially center on the way people working in the laboratory perceived their work, their colleagues and fellow employees, and the laboratory organization itself. Specifically, the data will include information about (1) beliefs about the way different kinds of people at AERL should behave, (2) beliefs about the respect due different people, (3) beliefs about the way the laboratory staff should be organized and specialized, (4) beliefs about leaders and leadership, and (5) data on the personal goals of individuals.

These beliefs and goals were a vital force in the everyday lives of the men who made up the staff at AERL. Indeed, the AERL organization is of particular interest to students of management because of the degree of emphasis people at this laboratory placed on the development by each individual of a personal set of guidelines and values that might best relate their behavior to that of others in the organization. This emphasis contrasted with the practice of allowing an impersonal technology or economic structure to dictate the behavior of industrial scientists. This emphasis was, furthermore, (in sharp contrast with many other organizations) pervasive throughout the AERL organization from top to bottom.

The cases are divided into two groups. The first group outlines the structure of the social beliefs and values that appeared to characterize the

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personal orientation of a large majority of individuals at AERL. Specifically, the

(*A*) case outlines the history of AERL and its organizational form in 1963
(*B*) case outlines the beliefs and values of AERL's scientific and engineering staff in regard to how members of this technical group were expected to behave and relate with others
(*C*) case oulines the beliefs and values of all groups at AERL in regard to the expected behavior of people in the technical and administrative service groups
(*D*) case outlines the beliefs and philosophy of several key leaders.

The second group of cases continues to outline the system of values and beliefs that characterized the working atmosphere at AERL, but also shows how these factors took operational form in some behavioral situations and settings. In this second group there is a

(*E*) case which discusses one key area of cooperative scientific work between scientists working at AERL; the committee meeting. This case discusses the nature of these meetings as people at AERL saw them and presents a record of the events at one meeting
(*F*) case which discusses the way in which people cooperated in satisfying the personal goals of professional and personal development
(*G*) case which outlines three situations involving conflict at AERL, and details how these were faced and resolved by management and the other people involved.

Early History of AERL

The decision to develop the laboratory now known as AERL was made at a cocktail party, in the fall of 1954, which was attended by Dr. Arthur Kantrowitz and Victor Emmanuel, the latter at that time AVCO's chairman of the board. Mr. Emmanuel had had close ties with Cornell University and was familiar with the work and reputation of Dr. Kantrowitz, who was at that time on the faculty of Cornell's Graduate School of Aeronautical Engineering. During the cocktail party Dr. Kantrowitz told Mr. Emmanuel that he was certain, based on work he had done with shock tubes, that the ICBM reentry problem could be solved. Mr. Emmanuel, interested in having the AVCO Corporation enter the missile field, consulted with other people at AVCO and made the decision to support work on the reentry problem under Dr. Kantrowitz. At the time, the solution of the reentry problem was of very considerable national importance.

Once the support of AVCO was assured, Dr. Kantrowitz made some telephone calls and in a few days, on the basis of his reputation in the field, attracted a nucleus of highly regarded technical people to work on the reentry project. Many of the men in this early group were or had been his own students at Cornell. This group of scientists immediately started work on the reentry problem, using the facilities at Cornell. At the same time, work was begun on the construction of a permanent laboratory in a

rented building in Everett, Massachusetts, a location chosen for its proximity to the Harvard-MIT intellectual community. The Everett laboratory was completed in March, 1955, and the staff, with the exception of Dr. Kantrowitz, moved into it. Kantrowitz directed the work of the laboratory by commuting from Cornell about once every two weeks. Mr. Kennedy, the laboratory manager, recalled that the group would often work until the early morning hours during Kantrowitz' visits, evaluating work already done and planning work to be done in the following two-week period.

The fundamental solution to the reentry problem was achieved by this group within six months, although much research remained to be done. Dr. Kantrowitz refused, however, to assume responsibility for the development and production of the nose cones that resulted from this program. AVCO therefore set up another new division, the Research and Advanced Development division (RAD), at Wilmington, Massachusetts, to build the nose cones.

The organization of RAD raised some important policy issues for both AERL and AVCO as a whole. The primary issue concerned the autonomy of AERL and involved the choice between making AERL a separate division or having it become the research arm of RAD. Apparently the main concern of AERL people during this period was a fear of being dominated by what was to be essentially a development group. The situation was resolved by making AERL a separate division, a solution upon which Dr. Kantrowitz had insisted.

Once the fundamental solution to the reentry problem was found, Dr. Kantrowitz, who was then permanently at the the Everett laboratory in the position of laboratory director and was also by that time a vice president and director of the AVCO Corporation, began to look for new directions that would be interesting for both his group and the corporation. He had long been active and interested in the area of magnetohydrodynamics (MHD), which centers on the study of gases under conditions of very high temperatures and strong magnetic fields. Dr. Kantrowitz decided to pursue his interests in this field, and the work then started eventually led to both the MHD research committee and the MHD generator project. Further support for MHD work was obtained from a contract for advanced development work and research on an MHD electric power generator. The costs of the work were shared by the AVCO Corporation and by a middle western group of electric utility companies.

Organization

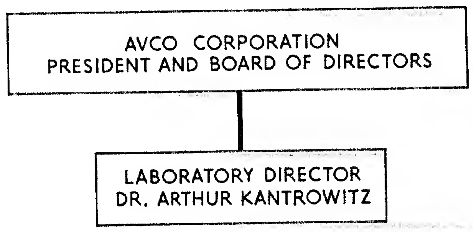
One of the first conversations the casewriter had with an employee at AERL was with the personnel manager, who outlined the current organization of the laboratory and many of its policies. Much of the material

below comes from this and subsequent interviews with him. One of the points made by this man, and emphasized by many others at the lab, was that there was no "organization chart" and that people didn't want one. The only organization chart to which people would agree is included in this case as Exhibit 1.

Exhibit 1

AVCO-EVERETT RESEARCH LABORATORY

FORMAL ORGANIZATION



Research Committees

The field of high-temperature gas dynamics was broken down into three major research disciplines and these areas formed the focus around which three standing research committees were formed: the MHD committee, the aerophysics committee, and the atomic physics committee. The formal function of these committees was outlined in the following statement quoted from a brochure used by AERL:

> The committees determine the general areas and objectives of research. In addition, they provide a forum in which current work is continually reviewed and discussed critically. Thus, individual staff members can benefit from the ideas of the group, and the group is kept informed of all work in progress. The interdisciplinary nature of the committee organization allows staff members to come in direct contact with all the disciplines represented in the Laboratory—an important educational advantage.

These committees did not have either clearly defined membership characteristics or delegated positions of authority. Committees at AERL were organized in order to differentiate the scientific work of the laboratory, not the scientists. These groups were essentially attempts to segregate the broad field of high-temperature gas dynamics into three separate, but not mutually exclusive, subject areas. As such, the committee structure was not a list of people but a list of topics. Only to the extent that men identified themselves with these topics were they identified with any of the committees. Even the committee chairmen were free to identify themselves, as individuals, with topics that were generally seen as being part of the subject area covered by other committees. Since no assignments were made to these committees, individual research staff men were free to

define the work they would do and the approaches they would take to the area of high-temperature gas dynamics.

Each of these committees did, however, have a formally recognized chairman, who was described as follows:

> Within the committee there was one respected technical leader. He was not known as an administrator when he became chairman nor does he have any formal administrative duties. This man was not elected, people just realized that Dr. so-and-so or Mr. so-and-so was the most qualified man within the technical area and later he was confirmed by Dr. Kantrowitz as committee chairman.

The senior staff members contributed in each area of research through regular committee meetings. Since membership on all these committees was completely open, any staff member could attend and contribute at a meeting of any committee. These meetings were the primary avenue by which the work and progress of each scientist was communicated to and coordinated with other scientists. Each meeting had an agenda, drawn up by the chairman, which consisted of progress reports made by individuals working on topics included in the subject area of the committee. The meeting itself was devoted to the presentation of progress reports and critical (often described as "bloody" or "brutal") *scientific* review of the work presented. By using this procedure, the staff and management hoped to fulfill a multitude of goals, including (1) bringing each other up to date on work done at the laboratory and elsewhere, (2) correcting errors, (3) channeling work into more constructive and cooperative paths, (4) encouraging individuals to treat their work as something apart from their personal worth, and (5) educating new staff members and technicians on the scientific foundation of the work carried out at AERL.

The role of the research committees was emphasized by several people at the laboratory, who noted that these committees controlled the work done in the laboratory even though there were neither clear calls for decisions to be made nor voting on issues. The way this control seemed to operate was that the group present at a meeting informally arrived at a position or a set of positions on a particular matter. The senior staff man whose work was involved was then expected to interpret this consensus and the sentiments underlying it, and take action based on his understanding. Dr. Kantrowitz also used the same mechanism (interpretation of an informal consensus) in making his own decisions.

The ultimate function of the research committees was to submit reports on their research results and to suggest possible applications of these results. If a particular area looked interesting to the corporation or to the government, and it appeared as though it should be taken to the application stage, these agencies would proceed accordingly, or the laboratory would handle it as a project until the area of work became large enough to be split off from AERL.

Projects

The particular projects that were active during the case writing period included the:

a) Magneto-hydrodynamic generator project (MHD), an attempt to generate electricity commercially from high-temperature gas flows.
b) Reentry experiments operation project (REXOPS), an attempt to increase the penetration of enemy missile defenses, while improving the defenses against enemy nose cones.
c) Superconducting coil project, an attempt to design and construct magnetic coils which operate under conditions of very low temperatures in order to reduce their electrical resistance.

These projects were somewhat different from the committees and were organized and operated more in line with general industrial practice because of their contractual responsibilities. The differences that were reported were primarily in the area of degree of supervision, responsibility for costs and deadlines, and in the more line-oriented organization of the projects. However, since most of the top people in the projects had started working in research committees, they apparently were reluctant to lose what they considered to be an excellent pattern of operation. Therefore, they tried to incorporate many elements of the committee system into their operations.

Administrative and Technical Service Groups

The remaining two groups at the laboratory were the administrative and technical service groups. James Kennedy, the laboratory manager, was in charge of the administrative group. Mr. Kennedy was originally hired as a technical staff member. When Dr. Kantrowitz decided that an administrative man was needed for the laboratory, he informally polled the lab's staff and Mr. Kennedy was unanimously chosen for the job. When Kennedy did an outstanding job in this role, Dr. Kantrowitz made him laboratory manager and delegated to him administrative responsibility for the laboratory. Dr. Kantrowitz, however, retained an interest in several administrative and financial areas even though his role at AERL was that of an active scientist. Dr. Kantrowitz expressed great confidence in Mr. Kennedy on administrative matters.

The underlying philosophy of both the administrative and technical service groups was that it was their job to free the technical staff for technical matters by providing needed services for them. The philosophy of "service" was very pervasive and firmly held in these two groups. It is worth noting that the technical service groups were formed to provide service for the research committees and when project work began to interfere with the committees' need for technical service, separate groups were established in the projects.

A formal standing committee, called the senior staff council, discussed policy matters that related to the lab's operations and made decisions on these matters. The members of this committee were Dr. Kantrowitz, Mr. Kennedy, the three research committee chairmen, the three project directors, the contract manager, the controller, and three members of the senior technical staff. These men discussed such issues as the reorganization of the senior staff, the establishment of a new financial information system, and arrangements for building a separate building to house the REXOPS project. To facilitate such discussions the council often appointed "action committees" to study a question in detail and report back to the council.

Personnel Policies and Practices

The basic organization of AERL was an outgrowth of both its academic spawning ground and informal developments within the laboratory that were given formal recognition. As the laboratory grew, its organization formed and became more and more stable. A basic differentiation between certain groups of people was made when it was noted that certain people formed a core of important contributors. Since these people were informally recognized as the technical leaders of the laboratory, Dr. Kantrowitz decided to make this differentiation formal and established the rank of "member of the senior staff." As the laboratory continued to mature, this designation eventually cut across all lines of the organization and came to include administrative and service personnel as well as technical people. The main group of influential people continued to be the senior technical staff since the laboratory's output was a technical output. Exhibit 2 is a breakdown of the employees at AERL (as of July, 1963) by position and type of work.

Recruitment for the senior staff was a somewhat unusual process. This process was, in the eyes of most people at AERL, a very important one, and to some it was the main explanation for the high degree of maturity which was, they believed, an important characteristic of AERL personnel.

The process placed great stress on a fairly long and informal acquaintance with a particular recruit and his work before an invitation to visit the lab was made (or before a staff support member was promoted to the senior staff). This acquaintance might have been made through direct contact with a man, or on the basis of his published works, and/or through contact with scientists who knew him. As an important part of the evaluation procedure which took place following such an initial visit, the recruit was asked to give a talk at the laboratory based on his work to that date. Some people claimed that since these meetings were like committee meetings (the same frankly critical reactions were evidenced at such a talk as were observed during committee meetings), one could tell

Exhibit 2

PERSONNEL BREAKDOWN FOR THE AVCO-EVERETT RESEARCH LABORATORY

KIND OF PERSONNEL	AREA: RESEARCH	PROJECTS	ADMINISTRATION	GENERAL SERVICES	TECHNICAL SERVICES	TOTAL
SENIOR STAFF:						
PH. D.	17	10			1	28
PROFESSIONAL	7	8	7	2	3	27
PROJECT AND RESEARCH-TECHNICAL:						
PH. D.	8	3				11
PROFESSIONAL	11	82				93
NON-PROFESSIONAL	1	110				111
TECHNICAL SERVICES:						
PROFESSIONAL					52	52
NON-PROFESSIONAL					193	193
OTHER			71	120		191
TOTAL	44	213	78	122	249	706

how a man would fit into AERL if hired. After such a meeting and several personal interviews with individual senior staff men, the recruit was evaluated by all senior staff men interested in making an evaluation.

Dr. Kantrowitz, on the basis of his impression of the group consensus, then made a decision regarding the man and his terms of employment. He also added a few personal criteria to his evaluation of a man, which he described as follows:

> I won't hire a man that wants to work 50 percent on company projects and 50 percent on his own projects. Other people will, but not me. In other words, if this field of physical gas dynamics is not a man's primary interest, then he should work elsewhere. I'm not going to hire men who are only going to work with me with their left hand. Some people here work 100 percent on the thing they want to do most and yet it's allied with the laboratory's goals.
>
> Also, before we decide to hire a man, I tell him to design a job that he would think was the optimum one for him. If the design fits here, okay; but if not, I'll advise him on where he should go to satisfy this design.

Several people pointed out that this hiring procedure also served the important purpose of acquainting the senior staff with work done and approaches taken outside AERL. One staff member noted that in some cases a mutual decision was reached that employment would not be a good idea, but nevertheless the man would continue to keep in touch with AERL on technical matters and occasionally would report on his work.

This same staff member also noted that the senior staff took particular responsibility for men elected to the senior staff, and that each new man had one or more informal but recognized sponsors. This sponsor had usually had some previous contact with the man before he was invited to give a talk on his work.

Chapter 33 (Continued)

AVCO-EVERETT RESEARCH LABORATORY (B)*

THE AVCO-EVERETT RESEARCH LABORATORY (AERL) was, in 1963, an organization which placed minimum reliance on formally prescribed activities and relationships in the conduct, coordination, and control of its work. Instead, the management and employees of AERL placed a maximum reliance on informal organization of their work and relationships. In other words, the operations of the laboratory were based on a system of norms, values, and interpersonal relationships which had gradually developed among the laboratory's personnel rather than on a set of rules, regulations, or controls imposed by the laboratory management. This informal system clearly expressed the way the large group of key men (the senior staff) at the laboratory wanted their own working lives organized and how they believed men associated with and engaged in research and development should behave and relate to other people.

AERL differed from more formally organized groups in many ways. For example: One well-known characteristic of a formal organization is the division of the organization's personnel into specialized groups which have fixed responsibilities. This specialization and definition of job function often takes the form of organization charts and written job descriptions. At AERL people were emphatically proud of the lack of such organization charts and job descriptions. There were, however, some broadly defined but specialized units within the laboratory (research committees, projects, administrative groups, etc.) and to this extent a formal organization did exist. However, it was clear (1) that the assignment of individuals to these units was based on the natural interests and capabilities of the individual; (2) that people were relatively free to migrate from group to group on the basis of interest and competence; and (3) that the nature of the work done in several of the specialized units overlapped the work done in other units. The ambiguity and continual change inherent in these situations was an accepted and expected part of life at AERL.

AERL also differed significantly from more typically divided and specialized organizations in that there appeared to be a minimum of fixed and defined responsibilities either for organization units or for individuals.

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For example, a man interested in and capable of doing basic research was free to contribute to the work of any of the four major research committees.

Another commonly cited characteristic of formal organization is a hierarchy of accountability and authority. This characteristic finds common expression in organization charts which show layers of supervision built into a pyramid with increasing authority and influence toward the top. The organization of AERL was, however, a shallow one with only a few layers of supervision and with many people at the same organizational level. For example, the top of the pyramid at AERL was represented by Dr. Kantrowitz, the laboratory director, and the next layer was composed of 53 senior staff members who all had equal formal status. The status of all senior staff members was, however, "equal" only in formal terms. Each man was accorded a degree of personal respect by his senior staff colleagues on the basis of his demonstrated competence and his value to others as seen by these same colleagues.

A final characteristic of formal organization is a set of fixed operational rules and procedures, and fixed standards of evaluation. AERL personnel were proud of the absence of a laboratory "Policies and Procedures Manual." The approach taken at AERL could be called "situational" in that the nature of each problem or issue defined the approaches and resources that would be used on it.

Values Associated with the Technical Work

The values held by the employees of AERL were closely related to the nature of the organization's goals as these were defined by those same employees. Two task-oriented goals were informally recognized at AERL; they were (1) primarily, the advancement of man's knowledge about his physical world through the discovery and widespread dissemination of scientific findings, and (2) secondarily, the investigation, when necessary, of advanced scientific concepts for their possible usefulness in producing practical and useful equipment.

Interelated with these two task goals were two other objectives which guided the work done toward these goals. These objectives were (1) the enhancement of both present and potential profit, and (2) the enhancement of the individual technical reputations and skills of AERL employees.

Since the primary task at AERL was defined as research, and since the original and several of the most highly regarded employees were researchers, the most emphasized value system at the laboratory was that connected with the conduct of research work. This value system was also applied to and accepted by people working in the projects which carried out the second technical task of the organization. These values affected the nature of the technical work done by providing the basis of a strong

code for the behavior of groups and individuals. This code was a set of unwritten rules or norms which outlined acceptable behavior, behavior which would win the respect and approval of the laboratory's staff and which supposedly would lead to successful technical work.

The following is a summary of the more important values and norms which made up this code:

a) Every technical man should be *committed*, on the basis of his *own natural interests,* to doing research and development in the field of high-temperature gas dynamics.

b) Responsible and competent members of the research staff should be *free to choose* their own areas for investigation. Responsible and competent employees should be free to choose their own methods for working on the tasks they faced.

c) The individual should *take the initiative* in planning his work and in guiding all his working relationships with other individuals and groups at the laboratory.

d) The individual should be *deeply involved* in his work, but should remain flexible in his approach and *open to constructive criticism and influence* from others.

e) Individuals and groups should *not allow personal feelings* to affect their working relations with others.

f) Everyone should be able to *communicate* their ideas, even though they may not be completely tested, in an articulate and well-organized manner to other interested personnel.

Two factors concerning this set of values are important to mention. First, this list was far more than a set of "nice sounding words." Instead, these values were a living guide to the everyday conduct of the technical work. This point was amply demonstrated throughout the case research period and is important because a surface examination of this code would show it to be little different from the standard norms associated with academic science. It is also quite similar to the phrases used by many industrial researchers and research managers to describe how they think their organizations should be or are operated. AERL differed from many of these groups in that these values were operational and were almost exclusively depended on to direct the scientific and technical work at the laboratory.

Secondly, AERL presented an organizational image to the corporate, scientific, and industrial communities that closely paralleled these values. Thus the laboratory (1) took the initiative in determining the nature of its technical work and strongly sought to defend the right to do this; (2) frequently invited critics of and contributors to the technical work at AERL to visit the laboratory and to make presentations to its employees; and (3) maintained a policy of maximum openness and frequency of publication in technical journals.

The casewriters accumulated a good deal of data which tended to support the value system outlined above. The remainder of this case is a summary of these data.

Commitment to Field of High Temperature Gas Dynamics. James Kennedy, the laboratory manager, underlined the importance of the norm that technical work done at the laboratory should be associated with the field of high-temperature gas dynamics by noting that people whose interests did not lie in this area were asked to leave. Dr. Kantrowitz stated that this norm was "at the heart of this lab" and Kennedy illustrated the point with the following case.

KENNEDY: There was one man here who just didn't seem to be at all oriented to working with other people and communicating with them. He was very much intent on only working in his own directions. Now as far as we are concerned around here we just don't do that. If you want to only work on your own interests, then you don't work here. When it became evident that this was the case with this man, we let him go.

INTERVIEWER: How does this attitude fit with some of the other values on freedom that I have heard here at the lab?

KENNEDY: Well, actually one of the central concepts around here is that we pull together as a team. You can work on what you want within the team's area but you have to be part of the team, and if you're not, then we don't want you. In other words, your goals should be in line with the goals of the team.

Another value that emphasized agreement on group interests was more subtle. The only man who directly expressed and emphasized it in interviews was Dr. Kantrowitz. This was the value that all technical work carried out at the laboratory should be potentially useful to society. Dr. Kantrowitz expressed his belief in the importance of this value as follows:

My whole attitude toward science is related to its impact on society. But to many scientists this relationship is a nuisance, and these are not my type of people. Scientists who have this nuisance type of attitude towards science and society relationships will find themselves in real difficulty around here.

Individual Responsibility and Initiative, and the Need for Impersonality. The values summarized above also placed great stress on the need for an individual to earn his freedom through demonstration of his sense of responsibility and technical competence. AERL researchers believed that the individual created his own environment for work satisfaction and growth. This belief, in turn, led to the strong value placed on individual initiative at AERL. The importance of this value was demonstrated in the behavior of people throughout the case research period and its influence will be apparent throughout this series of cases.

Closely associated with the support for individual initiative was a sentiment which emphasized the need for technical pioneering. This value was closely associated in the minds of many people at AERL with the influence and personality of Dr. Kantrowitz. For example, several people noted that Dr. Kantrowitz consistently pushed for new ideas and new

approaches to old problems at meetings of the research committees. A more general expression of this value appeared in the phrase used by many people to describe senior research and project staff members—"prime originators." This emphasis on pioneering was also evident in the feeling expressed by many people that the philosophy of management at AERL was a pioneering effort and that it represented a unique approach to the problems of R & D management. The pride many individuals took in this perceived uniqueness was very evident.

Hard work and a deep personal involvement in one's job was given particular emphasis at AERL. However, coupled with this deep involvement, a man was expected to be open to better approaches or to the perception of mistakes in his work when others brought them to his attention. These attitudes are summarized in the following comment:

> Well, there certainly is a personal involvement here and there damn well better be, and an individual had damn well better fight for his ideas because if he doesn't, no one else will. The orientation we do take here, however, is that the individual can be swayed if logic shows a better path. This is not a "stick to your guns come hell or high water" approach.

Despite the strong emphasis on initiative, deep involvement in one's work, and in fighting hard for what one wanted, people were not expected to allow their personal feelings to distort their perceptions of any technical information they received. In fact, many people believed that all feelings should be excluded from all issues and relationships, and many others appeared to believe that they should have no adverse personal feelings towards any other individuals or groups at the laboratory.

This "norm of impersonality" was illustrated by the following comment about the research committee meetings:

> These meetings can be very brutal and bloody at times but people are expected to attack a man only on the technical issues and avoid any personal references. In this way a man shouldn't feel he is being hurt and should find relations with others afterwards as friendly as ever.

Open Communications. In order to maximize the information and constructive criticism a man might receive, and also to minimize wasted effort, the value system at AERL placed strong emphasis on communication. A man was expected to communicate his thoughts and ideas freely and at all stages of his work, not only formally but informally. This emphasis on free and open communication also appeared particularly functional in light of the practice of determining individual responsibility and status informally on the basis of demonstrated interests and competence.

The importance of communication can be gathered from the fact that a lack of effort in this area was the one possible fault in a man that his colleagues would criticize severely. This attitude can be gathered from the following comments by a committee chairman:

People who don't communicate very well have a rather tough time here. Now there are people who can't communicate while they're formulating a problem even though they may be able to turn out an excellent report after the problem has been finished. I don't think we have many such people here. We tend to legislate against them by the way we operate. You see the committee meetings emphasize discussion of what you're doing. If a man doesn't communicate, he's just out of the stream of things. He's not part of the group.

Dr. Kantrowitz also indicated the importance of communication in the following comments:

I'd just as soon have a man go some place else if he doesn't want to contribute to our work here. After all if a man is just going to sit off by himself and only turn out a paper a year, I can read his paper just as well in a technical journal. In that case he is just as helpful if he were sitting in a university some place instead of sitting here, so why should I hire him?

This emphasis on open communication was important not only in reference to participation at formal committee meetings, but also in regard to open participation in the informal channels of communication. In fact, many individuals mentioned that it was through the influence of casual conversations in hallways, offices and the cafeteria that most of the work of the lab was done. This informal mechanism was given important emphasis by many people at AERL.

A corollary of the norms on communication and initiative was found in the frequent allusions to the need for people to be "vociferous." For example, one man said: "If you want something here you have to yell loud, and you expect your colleagues to yell loud if they want something badly enough." Another man also noted that "If you want to be violent, and I emphasize *violent*, you can always get something brought up at meetings here." In very few instances, did individuals appear to feel that the need for a "loud voice" was contrary to their own personal values.

Summary of the Values Associated with Technical Work. In summary, the social code at AERL demanded that an individual: (1) make a strong commitment, based on his own *natural interests*, to the field of high-temperature gas dynamics; (2) take the initiative in choosing his personal approach and in some cases his personal problem within this broad field; (3) become deeply involved in this approach and fight hard for his ideas; (4) constantly, despite his involvement, seek new approaches; (5) communicate his ideas and results to a critical audience in such a way that he was completely open to influence and change; and (6) accept criticism and return it without feeling in any way that he was being criticized as a person or in any way allowing personal issues to enter the defense of his position. (It seems appropriate to note here that one respected senior staff man noted that AERL was the "most brutally demanding place" he had ever worked.)

This code appeared to be capable of regulating behavior at the laboratory because most of the key people personally ascribed to the values

underlying these norms and based the respect, approval, and help they gave to others on how well the behavior of these other persons conformed to these norms. One man supported this impression by stating that "The thing holding us together here seems to be the extreme displeasure of people here to being left out of what's going on." Another man referred to the strength of this code in controlling behavior at the laboratory by noting: "There certainly is a very stringent organization here even though everybody says that there's not. This is one of the most highly organized places I have ever been."

A committee chairman also noted that the pride an individual took in "his own work" and the importance of his personal reputation to a successful scientific career provided a basis for the strong influence of "social pressure." Apparently a man at AERL would receive major help on his work only if he demonstrated his sincere belief in the values underlying the laboratory. The strength of this code and the unanimity of its acceptance was particularly interesting in light of the fact that it was nowhere given explicit and full written or verbal expression. Instead, people were expected to have it or develop it as part of their own personal beliefs. One other possible reason for the implicit nature of this social code was given by a senior administrative staff man as follows:

> This is a basic point. I think you will find the answer to this in the fact that scientists believe that as soon as you put something on paper you're just making paperwork and this is your main motivation in life. I think this is a psychological feeling that you are headed toward Parkinson's Law, which is very well known around here. So if you can maintain an informality, psychologically, at least, you have eliminated the dangers inherent in scientific people thinking that the organization is growing only for itself. They also recognize that we are unique, and of course it is rather hard to define just how we're unique, but as soon as you seem to be making formalities you seem to be threatening this uniqueness and indicating a direction toward conformity.

Status System at AERL. The formal organization of AERL defined all senior staff members as having equal status. This definition, in turn, reflected a strong value-supporting equality of opportunity. Furthermore, although variations in the degree of informal status accorded different individuals were recognized and desired, such informal status was ideally not associated with any degree of authority or accountability. An example of this that was frequently given involved the post of committee chairman. This post was given to the man who had achieved informal recognition for his technical leadership but people emphasized that these men had no "rank they could pull" on men in their committee.

One senior project staff member described the functionality of the "equality of opportunity" value and of the "shallow" organization which supported this value as follows:

> Now to my way of thinking, organizations have a tendency to be hierarchically and rigidly defined. Such organizations have a tendency of keeping people

from working where they are able to go. In other words, when there's no opening above their present position, people have to work below their abilities. Now I have seen this in other organizations where I was one of the people just below the cream of the crop. Without the positions into which I could move, where new challenges and areas for growth lay, I was effectively working below my abilities. Now without an organization this problem doesn't exist.

Another point is that around here you are forced to coordinate your efforts and to get along with the other people because there isn't any boss around here to resolve the conflicts that might arise. In other organizations, where there is a boss, you can afford to stand on your own two feet without really paying attention to the argument and stick to your own guns because the boss is the one who is going to resolve the situation. However, around here you cannot afford to consider your own guns only.

We also believe very much in keeping our relationships here open and flexible. Once you formalize a relationship by giving a man a title you will find it very difficult to change that relationship if later conditions indicate that you should.

However, although the organization was shallow it was not without some hierarchy. It was furthermore true that a man's position within the hierarchy that did exist did, to some extent, determine his behavior and the behavior of people toward him. For example, one senior project staff man noted that some technicians who had been working in the Ascension Islands in the South Atlantic Ocean, thought that when they were returned to AERL in Everett it was for rest and recreation. The senior staff man noted that "we soon disabused them of that idea." When asked how these men were "disabused," the senior staff man replied:

STAFF MEMBER: Well, actually, you just level with them. You just tell them the facts of life.

INTERVIEWER: How does this square up with what you've said about "You can't boss people around here, you can just convince them"?

STAFF MEMBER: Aha—well, maybe there is a double standard here (pause). Now, in a line capacity you have to sometimes take people to the verbal woodshed.

INTERVIEWER: Well, don't you have people over you in the line capacity?

STAFF MEMBER: I imagine so, but I don't think any of us would try—well, at a certain point you can see the woodshed gets dangerous. The woodshed is likely to make a man leave, and that's perhaps why we do operate by unanimous action here. You see, to force action by any other means, you could wreck the organization we have here.

It also appeared possible that the behavior of men holding the same formal status was not in full accord with the values of equal opportunity and the perceived lack of authority. For example, the (C) case will outline several procedural tools through which the committee chairmen could exert a strong degree of control over work in their committees.

James Kennedy also indicated a departure from the ideal that informal

status was not associated with authority or accountability when he noted that:

I guess that we have a kind of totem pole within the senior staff. Sometimes a man down the line will ask one of the top scientists here to do some work, but when this happens the committee makes it pretty plain that the man down the line is the one who should do it and it invariably works out that he does it.

Now it works the other way too. A top scientist could ask a man down the line to do some work, and in this case, when the fellow at the bottom is asked, I guess he is flattered, and most people will go all out on this basis and will do the work. We don't usually find a man who turns down this type of assignment.

One member of the senior staff believed that informal status was *not* free of authority implications and believed that this discrepancy could be disfunctional to the laboratory's work. He noted that he had faced serious internal opposition to his work at the laboratory. He noted that he felt this opposition stemmed primarily from the fact that he was not one of the original "Cornell group" at the laboratory—what he called the "first team." This senior staff man explained this situation by noting:

You know Kantrowitz does tend to rely on his students quite a bit more and accepts their advice quite a bit more than the advice of other people. This is natural because he knows how they were trained and he knows each of them quite well. But it sure makes it harder for others. As a matter of fact, several people have left because of this very situation.

Dr. Kantrowitz appeared to be quite sensitive to this criticism of a "Cornell team" at the laboratory and stated that he had explicitly gone out of his way to bring in people from other schools.

Another factor concerning the status system at AERL centered on the position of scientific coordinator. Every contract at the lab had a scientific coordinator who kept track of the contract's budget and incurred costs. One member of the senior administrative staff noted that the position of scientific coordinator was, to his way of thinking, taking on status implications. He described these feelings as follows:

STAFF MEMBER: In the past we had no such thing as a scientific coordinator, and in fact the word "project director" was a very nasty word. These words were just not tolerated. But we now tend to see that if a scientist tries his hand at this scientific coordinator role, he begins to like it. He seems to like the practical authority and also the prestige and the status, but mostly, I think, the authority. They learn that science is not all that a person can be interested in and they see the value of other interests.

INTERVIEWER: Well, from my experience they don't seem to admit this—if they do like it.

STAFF MEMBER: Yes, that's right. They *don't* admit it. When they first take over this job as scientific coordinator, they resist it bitterly. But if you watch them, you see that they do like it. This puts them in a position of saying to another man, "Well, I can't let you work on my project; I

just don't have the money for it," or "I'd like you to work on my project; I have plenty of money." Even the title seems to give some a good feeling.

INTERVIEWER: Why do you think that they deny such interests if, in fact, they do exist?

STAFF MEMBER: That's a real interesting question and I don't know the answer. I've often tried to think of it. I don't know if it's part of their formal training—are they *taught* to think this way? But I do see many coordinators many times go up to scientists and say: "Your progress is not good, and I'm paying for it, and I don't like it."

Informal Organization and the Advanced Development Projects

The informal system outlined above was described as being the primary organizational system for technical work at AERL. However, as noted in the (A) case, this technical work was divided into two main categories, consisting of (1) research work carried out within the committee structure and (2) advanced development projects [the reentry experiments operation (REXOPS), the magnetohydrodynamic (MHD) electric generator project, and the superconducting coil project]. It was clear that the informal system was indeed the primary organizational system for research work. Indeed, many people noted that since the laboratory had originally been devoted only to research, and that since Dr. Kantrowitz identified himself most closely with such work, this informal system was originally developed in a research context.

However, during the course of the case-writing period, many people at AERL described the projects and committees as "basically different." Members of the research staff, for instance, noted several differences between their committees and the projects. Several people noted that the projects were organized more in line with standard business practice, with fixed responsibilities for costs and schedules. One almost unanimous belief was that the projects were "line organized" whereas the rest of the lab was not. The project director was seen as exerting a strong influence on the work done in a project versus the emphasis on individual responsibility for and ownership of ideas perceived in the committees. Several people expanded on this last point by noting that Dr. Kantrowitz dealt with the work done in projects through his relationship with the project director rather than by relating to the individual scientist as he did with research work.

The casewriters soon realized that the three projects were very different, whereas the three committees had many things in common. The projects differed in regard to their degree of "hardware" orientation, relative need for research activity, degree of involvement in the main technical areas of AERL, the leadership styles demonstrated by various project managers, and, finally, their relative size.

The largest project at the lab was REXOPS which was concerned with the problems of missile defense and missile penetration of enemy defenses. It became increasingly apparent to the case writers that in many respects the perceived differences between REXOPS and the committees were less sharp than many had pictured them. Several people who worked on this project expressed very close agreement with and strong commitment to the values, norms, and goals expressed by people in the committees. For example, the head of the instrumentation group (the most widely named unit as the most "line organized" group in the laboratory) noted: "We don't like hierarchies here. The organization charts we make up for the customer are burned when he is through with them."

Another project staff member also noted that the management of the project was very much like that of the committees and supported this by stating:

> As far as the management of this program goes, we have a kind of self-organizing management structure. In other words, there is no one on top. People organize themselves into various positions based upon their competence. No one really worries around here who works for whom, etc. For example, I get a lot of over-all guidance from the project director, but on the other hand he works for me, on my contract, as a scientist. In other words our fundamental idea here is that we are organized to do the job we've got to do.

An interesting issue between REXOPS and the research committees arose during the case research period. It had become apparent that REXOPS would have some excess funds whereas the committees were in need of more funds. The research committees hoped, therefore, that the project would release some funds to the committees. However, the project director refused to make a decision on this matter. One senior staff man at the laboratory described the resulting attitudes as follows:

> Now the reaction by people on the research side is that the project director here is not being the hard and fast manager they believe he should be. Instead, they see him as operating under the philosophy of running the project in a very loose way, with considerable amounts of freedom for his people. In other words, this project director is seen as not making the decisions that a project manager should make. Actually there is a very interesting switch here between the ways you think about the project people and the ways you think about the research people. Dr. Kantrowitz is also playing a rather interesting role in this situation. Arthur's course is that of finding another more natural solution; he is hoping to find some money in other contracts that could support the research staff.

In another interview a member of the senior project staff seemed to be expressing a stereotyped attitude towards the research scientists. For example, he noted that these scientists were not really appreciative of and interested in the "facts of life: money and scheduling." Upon further reflection, however, this man noted that "There is a lot more fiction to these impressions than fact." He also supported the view that the basic values and goals of the two groups were quite similar. When asked how these fictional impressions had developed, this man answered:

I think that they're largely based on preconceived notions. In other words, I think we tend as people to go to the extreme and see the worst case in our relationships with other groups. I think that really when you get down to it, there's a lack of trying to see the other person in a real sense. People seem to start with a chip on their shoulder and talk about things as they think they are and pretty much work on these preconceived notions. However, I think once you have learned that the chip is disfunctional you won't do it again.

Another project at AERL was the superconducting coil project which was the smallest project and the one furthest from the main line of technical work at the lab. The fact that this project was concerned with very low-temperature phenomena, while the rest of the lab was working in the field of high-temperature gas dynamics, apparently was a strong influence on the role of this project at AERL. The result of this situation was that the project regarded itself as somewhat more autonomous than other activities at the laboratory and one that was more conservatively treated by management. The technical work done on this project apparently needed more justification than other work done at the laboratory, and the project was also apparently restrained from bidding on contracts until the project's capability was proven in detail. These actions by management at times seemed to frustrate the project people, even though they usually expressed satisfaction with the basic philosophies prevalent at AERL.

Chapter 33 (Continued)

AVCO-EVERETT RESEARCH LABORATORY (C)*

THE NORMS, values, and sentiments which governed the behavior of research personnel at AERL were shared to a considerable extent by the various administrative and technical service groups working in the laboratory. At the same time, the values held by these groups differed in a number of ways in accordance with the differences in orientations which inevitably characterized the relationship between research and service groups.

The casewriter observed, for instance, that whereas research people were expected to "take the initiative" in planning their work, individuals working in technical and administrative service were expected to live up to minimal (usually unstated) requirements associated with the functional identification of their positions (personnel, mechanical design, maintenance, and the like).

Similarly, the clear requirement that communication between research people be "open" was less clearly a universal norm among technical service personnel or between technical service and research people. A certain amount of tension was generated between the technical service and research groups, in fact, because of the belief in both groups that, in matters involving cooperation between them, "the customer (the researcher) was always right." Service people contrasted this attitude with that on which the norm of "open communication" among researchers was based; namely, that "the party who is right is the one who proves his point." Those working in the service groups sometimes felt it was very difficult to prove their point because the researchers appeared unwilling to listen to them or to try to learn enough about the service functions involved to understand the position being taken by service personnel.

Although some conflict situations existed in regard to the role of the technical service groups at AERL, most of the technical service people appeared to accept their service role. For example, one man in the computing group told the casewriter:

> The main consideration here is that I'm running a service and I try to keep everybody happy. Our ground rules are that we are a support group, emphasizing support. This is our prime mission and we satisfy this within the rules of

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being fast, accurate, and doing our work at minimum cost. However, this spirit is not at the same level today, perhaps, as it was four or five years ago when we first started and had a smaller group. Maybe this slip-off has been a question of increase in size or more organization; it's hard to say.

Since this is a support group, the basic question or reaction I've had, especially from young cooperative students, is that they're not too sure they want to do this for a career. They seem to say: "You fellows are at the tailend of the job, which means that you don't know where your work fits into the job as a whole." Now maybe there is some truth in this, but there's also a significant feeling of getting up and helping other people out.

All of the administrative people interviewed by the casewriter placed considerable emphasis, either implicitly or explicitly, on a *service* orientation. For example, one man made the following comments about the function of the administrative service groups:

Scientists, you see, don't like to get involved in administration, and we try to take the administrative detail off their shoulders. In other words, we're trying to minimize their administrative work so that we will minimize the time they spend away from their scientific work, which is their main field of activity. This point of view of establishing an atmosphere that is conducive to optimum research is one of Dr. Kantrowitz' special outlooks on R & D management.

The administrative service groups at the laboratory were under the overall direction of James Kennedy, the laboratory manager. While Kennedy was not, in a formal sense, "over" the technical service groups, at least two of the men in charge of these groups considered Kennedy their "boss," and all of them, in the few cases where dismissal was necessary, depended upon Kennedy to "fire" people in the technical service area.

The Administrative Group at AERL

The Administrative Role at AERL. One classical area of administrative concern, sales, was not directly included in the role of the administrative group although they assisted in this function in the role of contract administrators. One man explained this in these terms:

The scientists are, to a very large extent, the salesmen in this laboratory. This is especially true because so much of what they do sell is on a highly scientific basis. In fact, this ground rule that the scientists are to sell the laboratory contracts was originally set up and is firmly held to by Dr. Kantrowitz.

Another classical area of administrative concern (one that is often cited as a cause of tension between administrators and scientists in industrial laboratories) was the question of space management. This problem might have been particularly acute at AERL because of a stated management policy of "living in last year's clothes." This referred to the fact that management purposely provided less office and lab space than was needed in order to avoid the "well-known effects of Parkinson's law." However,

the man who headed the committee in charge of space assignments was a widely respected member of the senior research staff who had the dual reputation of being a highly qualified scientist and having a well-developed sense of humor. One member of the senior administrative staff told the case writer that this scientist had voluntarily requested administrative work, had done an excellent job as chairman of the space committee, and that the administrative group was very glad to give this responsibility to him.

It has been pointed out that there were important perceived differences between the research committees and the advanced development projects [see Avco-Everett Research Laboratory (B)]. As might be expected, administrative staff people perceived differences in the ways they felt about and behaved toward the scientists working in committees and projects.

In dealing with the research committees, administrative people tended to emphasize the service orientation built into their roles. Their relationship with the projects was quite different. Here they saw their role as one of straightening out the project groups on management problems.

They believed they should and could do this because the projects were more "business-oriented" and were therefore similar to other industrial groups. For example, one man stated that in the past the administrative groups had spearheaded a drive for more organization in REXOPS. There appeared to be some concern that this project needed still more organization. This attitude was reflected in the following comment:

> This group certainly needs a central clearing house because right now they are only a collection of little empires with no coordination. Now at one point the administrative group was offered the job of coordination of REXOPS but refused because it would have been difficult to get the authority from the technical people in that area. The action committee of REXOPS is one way of theirs to get together to come up with conclusions, whereas in the past they have often not been together at all. This committee has worked out well and it has proven to this group that in the past they had a serious lack of communication.

A final and interesting role that the casewriter observed being played by administrative people was that of confidant to technical people who were having problems. This was observed in three cases. First of all, in regard to two of the conflict situations [described in Avco-Everett Research Laboratory (G)], an administrative man told the casewriter that some of the participants talked things over with him in the process of deciding on a course of action. In another conflict situation, involving one of the technical service groups, the personnel manager told the casewriter that he came to know about this situation because both major parties and the technicians in the group had visited his office to discuss the situation. Jim Kennedy noted in passing that he had also had such conversations. He furthermore noted, in discussing a research scientist who had been asked to leave the lab, that he was the one who did the asking because "everybody else couldn't bring themselves to do this."

The Administrative Atmosphere at AERL. The administrative staff had apparently been able to find a great deal of personal satisfaction in their role as service personnel. They appeared to believe that this role was a stimulating and challenging one. They were also proud of the atmosphere they had helped build and of the financial record of the lab. This satisfaction was present even though some staff members had originally had difficulty adjusting to the atmosphere at AERL. One man, for example, noted that he was quite unhappy when he first came to the lab. He stated that at that time he firmly believed in a strong organization of efforts with fixed and closely defined responsibilities. He noted that "I didn't want to stop and ask if this or that was my responsibility; I wanted it spelled out in black and white." However, the man stated that after an "all-night" talk with Mr. Kennedy, he "finally saw that an informal organization could also work." He also added the comment that "After all, you can't argue with success."

Another administrative man made some interesting comments about the relationships between the scientists and administrators, and how these relationships had developed. The casewriter had told this man that in his experience many laboratory administrative groups felt they were treated as "second-class citizens." The administrative staff man agreed that this was true in general but noted that it was not true at AERL, and that "getting along with scientists" was the most interesting part of his job. He then went on to discuss this issue as follows:

> There are some scientists, of course, who still look on us as a necessary evil, but we're quite glad that their number is very few, although a couple of years ago the majority of the people here felt this way. We've certainly made real progress in the last few years. To get this progress we had to move very, very slowly. We worked with the most susceptible people first. We certainly didn't move to the rebels first. When these more susceptible people became convinced of our value then they helped us in spreading our value. In a sense they became our press agents.
>
> As a matter of fact, I get visits each month from scientists. I don't have to go see the scientists any more in their laboratories. They actually come here and I don't have to make appointments and fight through levels of secretaries to get to see them as I had to in the past. This even goes for some that were orginially quite the rebels.
>
> However, we still have a long way to go yet in the way of introducing the technical people to the everyday facts of life. But they are intelligent people and it's a gradual process. We are confident that it will be accomplished, although we really don't know how we did accomplish it in the past because it developed so gradually.

The casewriters did indeed find ample evidence that an attitude of "second-class citizenship" was not an important element in the administrative-scientific relationship at AERL. It is only in light of this ample evidence that the following comments should be evaluated. These comments all were made by members of the administrative group and pertained to some as yet unresolved tensions in administrative-scientific

relations at the laboratory. One senior staff man, for example, said that:

The main problem in the administrative group is the feeling that we are becoming more and more aware of the technical goings on around the laboratory but that the scientific people are wanting to less and less become aware of and allow for the administrative aspects of this laboratory. This situation tends to become quite serious at salary review time, and the only way many scientists are able to evaluate our usefulness is on the basis of the personal favors we've done for them. From my point of view, this isn't really an evaluation, nor is it an evaluation based upon a real concept of what the job of the administrative staff is. Actually, we're kind of walking a tightrope here in which a very small criticism can mount up in the eyes of technical people. And it seems that at review time the only things that are mentioned are the adverse things.

However, there are overwhelming positive things here at the laboratory too. Certainly the opportunities provided for the administrative people through this type of management have given us all a great opportunity. So you see, it's certainly worth the small payment we have in any problems.

Apparently the administrative staff also felt slightly resentful about many of the younger men on the senior staff who had not worked in other laboratories. This feeling existed because these men were seen as taking the administrative staff and their hard work and dedication for granted. According to the administrative men, these young scientists assumed that all administrative staffs in all laboratories were the same as at AERL. However, it was also noted that some scientists who had worked for other laboratories were very much aware and appreciative of the importance and uniqueness of the administrative staff at the laboratory.

The casewriter received very few comments from scientists about their relations with the administrative group, even when he directly asked for them. However, the following interview with a senior project staff member is apparently typical of any attitude that existed:

STAFF MEMBER: Well, I've made it a point to meet these people halfway and to get along with the administrative staff. This is not just because I'm a nice guy. I have a rather selfish motive in this. I have to know their needs so that I can get the best work out of them. I don't see any schism here at all.

INTERVIEWER: I've noted in other laboratories, in fact quite a few, apparently, that the administrative staff is treated as rather second-class citizens.

STAFF MEMBER: That's not true here because you see the administrative staff at this laboratory doesn't try to run us and we don't try to run them. I think in the past, the few times when I've tried to run them, for instance by interfering with purchasing, I just got a phone call from them saying "Listen, do you really want to place your own orders?" When that happens I realize that I have to trust them and rely upon them for this type of thing.

Chapter 33 (Continued)

*AVCO-EVERETT RESEARCH LABORATORY (D)**

THE PERSONALITIES and behavior of the men who occupied important leadership positions at the Avco-Everett Research Laboratory (AERL) appeared to be a particularly important influence on the behavior of people working at the laboratory. This was especially true in regard to Dr. Kantrowitz, the laboratory director, who was seen by many people as having had an unusual degree of influence on the technical direction taken and on the establishment and maintenance of the value system at the laboratory.

The behavior of these leaders was also interesting because the emphasis on the values of equality and individual initiative built into the normative structure of the social system at the laboratory seemed to place special demands on the men in formal leadership positions.

The Leadership of Dr. Kantrowitz

One research committee chairman summed up his feelings about the important influence of Dr. Kantrowitz as follows:

> You can talk to 50 different people here and get 50 different ideas as to how this lab works. It doesn't really matter because the lab is a cult of personality. It centers around one personality, and Dr. Kantrowitz has a rather strong personality.

Most of those who made such comments, however, also emphasized strong feelings of respect and admiration for the laboratory director. It was apparent that almost everyone interviewed perceived him as a talented leader in both a technical and an administrative sense.

The casewriters gathered data on how Dr. Kantrowitz functioned in his leadership role from a number of people at AERL, including Dr. Kantrowitz himself. A rather colorful introduction to this subject was the following statement made by a member of the senior staff:

> When I first came here I got the smallpox sign, which meant that I was allowed to sit in and hear the great masters. At my first meeting, which was in the atomic physics committee, I tended to spot each person who contributed and could kind of gather what his field of speciality was. Then Kantrowitz

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spoke up, and I said "Aha, he is an atomic physicist." Then I next went to an aerodynamics committee meeting and once again spotted the people by what they were interested in and by what they said. Once again Kantrowitz spoke up, and I said to myself, "Aha, we're dealing with a double-threat man here." But when this happened a third or fourth time, well, it was just too much.

In other words, Dr. Kantrowitz is tops in all these areas, and although he may not be the best MHDer in the laboratory, he sure is better in 20 other fields than the best MHDer is. Furthermore, he can out-think, out-drink, out-spit, and out-swear anybody around here. And he has enough self-confidence to be able to surround himself not with second-run people but with the best available.

Dr. Kantrowitz' assistant was a graduate of the Harvard Business School. This man attended all the research committee meetings as their secretary and had an excellent opportunity to observe how Dr. Kantrowitz behaved at these meetings. The following comments by him seemed typical of the attitudes generally held at the laboratory:

Dr. Kantrowitz opens himself to the same type of criticism and correction as everyone else receives here. He doesn't mind being told when he is wrong—but he does mind if he's *not* told he is wrong when he is. He also doesn't hesitate to sharply criticize other people, but not on personal issues, and only if they are technically wrong. However, he does seem to be careful about too strongly criticizing younger people. I think that he restrains his criticism of younger men because he very much believes good scientists are made and not born. He shows this attitude by devoting special attention to the development of young staff men.

I guess what I'm saying is that Arthur is generally quite impartial when it comes to evaluating any technical data. Furthermore, his very presence at committee meetings seems to sustain this attitude of impartiality in others also. I may be wrong about this but I know that we certainly go out of our way to arrange committee meetings so that he can be present.

As a boss, Kantrowitz is different from any I've had. He won't tell you what to do and he won't tell you what he wants you to do. However, he will share a goal with you that he thinks you ought to achieve and one that you think you ought to achieve.

Now some people think he is also difficult to get a decision from. But you see, after he gets to know people, he is quite capable of relying completely on them and lets them make their own decisions. He also believes that people operate best under more pressure than they think they can stand. As a result, Arthur has the fairly common practice of over-committing the laboratory in terms of dollars, or contracts, or information output.

James Kennedy further illustrated Dr. Kantrowitz' philosophy of leadership by describing an incident that concerned a scientist who was having difficulty adjusting to the laboratory. At one meeting this man fought very hard with Dr. Kantrowitz over a technical issue. The scientist maintained his position and was able to prove Dr. Kantrowitz wrong. Mr. Kennedy said that the man was "flabbergasted" to find that, at bonus time, Dr. Kantrowitz had awarded him a bonus for winning this fight. After this incident the scientist performed well at AERL.

A member of the senior project staff, who was particularly pleased

with the atmosphere at AERL, indicated how he felt the personality as well as the behavior of Dr. Kantrowitz influenced this atmosphere:

> People around here are not at all interested in moving up a quarter of a point in the pecking order. In fact, there is no pecking order around here. The lack of struggle at the top is accompanied by the lack of struggle up and down. *There is no struggle.* Almost everyone in this place feels himself *completely* invulnerable and therefore we're quite relaxed about the position, or our status, or our salary. In other words, Kantrowitz' feelings about being open to competition and his own personal security and confidence pervade the organization up and down.

A final interview with Dr. Kantrowitz, after the rest of the data in these cases had been gathered, produced the following comments on how Dr. Kantrowitz himself perceived his role at the laboratory.

KANTROWITZ: I really have my foot in two different worlds here. In the first place, I'm here to make profits for the company, with a secondary interest in fulfilling a role in the nation's security. In the second place, my role here is to advance the frontiers of knowledge, with a secondary interest in knowledge for the purposes of national defense. Now the point is that the only contact between these two worlds is between their secondary interests. It's on this level of secondary interest that we have established the commitment of our people in working for the lab. That is, everyone recognizes the need to defend our country. Now of course the implication here is that this approach doesn't work so well in such industries as household appliances.

INTERVIEWER: Are you saying that it's easier to run a laboratory if you have some kind of superordinate goal, and in fact that this must be there?

KANTROWITZ: Yes, that's right. These two worlds of profit and knowledge have for a long time gone their own separate ways, so much so that it has become a matter of difficulty to get them together at all. Now you can run laboratories in such a way as not to bridge the gap. The typical industrial laboratory, for example, will frankly adopt the profit motive. However, I think that inevitably this type of organization will set their sights too low. Or you can take the other extreme, which is the extreme of the very isolated university research worker who lives on government grants, or the like. The end result here is scientific papers. Now we at AERL derive some excitement and enjoyment out of an attempt to bridge this gap between the worlds of profit and of knowledge.

Dr. Kantrowitz then noted that one of the philosophies which guided his behavior as laboratory director stemmed from Plato's observation that "What is honored in a country is practiced there." He noted that he applied this observation by relying on his ability to influence the climate

of opinion at AERL and that this was the major aspect of his role. When asked what he meant by this, Dr. Kantrowitz replied:

KANTROWITZ: Well, every scientist wants to do things that will enhance his position among his colleagues. If you can change the climate in which these people work then you can guide a "free" group. However, I use the word free in quotes because no one really is free.

INTERVIEWER: What are the important elements of the way that you influence this climate?

KANTROWITZ: Well, the committee meetings are really my opportunity to do just this. I give or withhold praise. I guess this is the critical element of what has been done. I also exhibit excitement when it is really aroused. Now I'd like to note here that other people are also important on this. I don't do it completely on my own.

INTERVIEWER: Isn't this quite a burden for you?

KANTROWITZ: Yes, but I regard it as a major part of my job and it is the only technique I know of for controlling a research activity.

Of course one of my philosophies in dealing with this whole question of leadership is that in general I like to have a situation exist and then recognize it—rather than creating the situation by any formal type of announcements.

INTERVIEWER: You'd rather not push things around here then?

KANTROWITZ: That's right. We'll push the climate of recognition and then we'll push the climate of opinion, and then, when the opinions have changed, we'll formally recognize the fact that they have changed.

INTERVIEWER: Dr. Kantrowitz, how much influence do you think your behavior has upon the people around here? We have the impression that it influences them rather strongly.

KANTROWITZ: I have the impression that I'm only a minor influence.

The casewriters also raised the question of who might be the successor to Dr. Kantrowitz in his position at the laboratory. Dr. Kantrowitz in reply referred to this question as "deadly serious" and the main problem of the laboratory. He further noted that:

> The committee chairmen would be the natural men to take this job, however, one of the problems with this is that I overshadow them for the time being so that it's difficult for them to really get the training necessary for this. I also think that it's very comfortable for the people in the research part of the laboratory to exist just as scientists. They have a fine umbrella over them and they really have no motivation to learn to fill any role such as a laboratory director. I would hope though that if they did get into this position that they would be able to grow once they landed in it.
>
> On the other hand this lab, to a considerable extent, can operate independently, and, as a matter of fact, it really does so now. I believe that the philosophy of management here is pretty stable, and that wouldn't change overnight.

Dr. Kantrowitz also noted that he thought the committee chairmen were "too smart" to take the role of director. He explained this remark

by alluding to the "inherent ambiguity" (by which he meant the "foot in the two worlds," discussed earlier) built into his role, which he saw as a serious obstacle to finding a successor. The casewriter then asked:

INTERVIEWER: You've mentioned this question of ambiguity quite a bit. Is this situation less ambiguous once you recognize the ambiguity?

KANTROWITZ: Yes, but it's very hard for someone who is solidly involved in some other role to recognize this ambiguity. It was a very difficult decision for me.

INTERVIEWER: I'm sure you've gotten your rewards for this.

KANTROWITZ: Yes, it's been exciting and I've gotten used to it. I guess the most important reward you have here is a broadening of your perspectives. But one thing that continues to bother me is that the scientific administrator is not a highly honored position in our society, and that's a very important thing to me. He's not at the top of the business community either, and I see this as a real detrimental part of this job. I think that so long as this is the case we cannot expect that the management of science in this country will be well done.

Dr. Kantrowitz then discussed in detail a plan he had for review of decisions of national importance, which had both a political and scientific side. This plan was essentially based on a separation of the scientific and political aspects of an issue. Once such a separation was made Dr. Kantrowitz believed that a group of scientific "judges" could impartially render a judgment on the scientific side. This judgment would then form a basic benchmark from which purely political arguments could proceed.

At the end of the above discussion he mentioned that questions involving national policy on scientific matters tended to "take up a considerable portion of my time now." The interviewer then noted:

INTERVIEWER: In other words, the laboratory in a lot of ways can support itself without your constant attention.

KANTROWITZ: Yes, but . . .

INTERVIEWER: But you have an interesting role here and in a lot of ways you're the "daddy." You protect the boys, you reward people, and so on. And this is probably a pretty darn important type of role.

KANTROWITZ: Right. It's a function I know I have to do and I believe that a significant part of the success of this lab depends upon how well I can carry out this function. This is true, for instance, in how well I protect the lab, and this depends upon connections in Washington. This is exactly why I get so involved with questions like the one I talked about involving national policy on science.

Several people had told the casewriters that they would have a difficult time interviewing Dr. Kantrowitz because the laboratory director tended to be pointedly modest and unwilling to speak about his ideas on personal leadership. These people pointed out that Dr. Kantrowitz would deny having any leadership talent although he knew he possessed quite a bit.

This prediction was borne out in the interview. Essentially, what happened was that whenever the casewriters asked Dr. Kantrowitz for an explanation of why the atmosphere at the laboratory was so good, or morale high, or relations between groups excellent, he gave an answer devoid of personal influence. In other words, he would explain the results in terms of such things as the need the laboratory filled in the nation's security, or the profits being received on contracts. When asked specifically about morale, Dr. Kantrowitz replied: "Maybe we're happy because we fill a need and because we bridged this gap."

This attitude on the part of Dr. Kantrowitz was in marked contrast to that expressed by most of the other people interviewed who, when asked the same questions, immediately pointed to (1) the influence of Dr. Kantrowitz' personality and (2) their own planned personal efforts over periods of time to organize, motivate, and direct themselves.

The casewriters obtained a final bit of data about Dr. Kantrowitz and the attitudes of at least some people about him from a member of the senior research staff. This man discussed the central position of Dr. Kantrowitz in the laboratory and agreed with others that this position was one of great importance. However, he also noted a departure from the views of many others: "that if Kantrowitz ever left, the laboratory would suffer greatly." This man presented a strong departure from this pessimistic view in the following terms:

> It certainly makes me mad when I hear people around here say that this lab would fall apart without Kantrowitz. It seems they have no confidence in themselves at all when they talk like that, and that they are contradicting some of the basic values here.
>
> To my way of thinking the lab would certainly go along on its own momentum for a time. I don't think it would fall apart; after all, one man is not *that* important. We respect him and are fond of him, sure, but I would not want to let myself become *that* fond of him. After all, I'm a big boy now and I certainly don't need a father figure to guide me all the time. No, I think that you'll find that this place has very little competition and there's no place quite like it; it fills a definite need in this country. The country needs a place like this, and our survival is based on this need—not on one man.
>
> You see, I like it here quite a bit, and I am conceited enough to believe that in some way I have been responsible for the lab's success. I certainly would want to keep this place going, at least based on my past experience. Well, then you might say suppose we did get someone who was not as good as Arthur. Well, then it's up to us to adapt to him and to help him become what we want.
>
> Now I guess it would hurt Arthur if I told him this. (Pause) No, on second hand, I don't think so. It probably would hurt him only for five minutes or so and then he would agree with me, or I at least expect that he would. But I would be bothered if Arthur wasn't hurt. I believe that the feeling of your own importance in the operation of the whole is an essential part of leadership ability.

Leadership Roles of Other Members of Top Management.

Several other important technical and/or administrative leadership roles existed at AERL; namely, the roles involved in the positions of

deputy director, laboratory manager, research committee chairman, and project director.

The Deputy Director. The deputy director of the laboratory was also the chairman of the atomic physics committee and was an eminent scientist who had an international reputation for scientific research. This man was also the most prominently mentioned candidate for succession to Dr. Kantrowitz. The deputy director, however, was apparently not sure he wanted any additional administrative responsibility. He tended to think that such responsibility detracted from a scientist's own technical research, and he appeared to identify himself mainly with this research. He also believed that his major contributions to the laboratory were the results of his own research and a backlog of scientific experience which he was willing to share with others at committee meetings.

It was apparent that both Dr. Kantrowitz and Mr. Kennedy were aware of the attitude of the deputy director. However, they believed that what he said was very different from what he practiced. Kennedy noted that he had seen the deputy director carrying out his role as a leader far too often and too well to believe that he was either unwilling or incapable of fulfilling an important leadership function at the laboratory. Kantrowitz also noted that the attitude of the deputy director was just one small example, among many, of a situation in "flux" at the laboratory. Dr. Kantrowitz (and others) believed the deputy director was gaining more and more acceptance of his leadership role and that this acceptance was developing on the basis of changes in the deputy director's own natural interests as he began to see more value in such a role.

The Laboratory Manager. James Kennedy, the laboratory manager, clearly agreed with the philosophy of management expressed by Dr. Kantrowitz, and his resulting behavior earned for him, too, the respect and esteem of his subordinates and colleagues. For example, one of the members of the senior administrative staff noted that:

> We all have considerable confidence in Mr. Kennedy and are quite proud of the system of freedom that Jim operates on—giving his administrative senior staff full room for initiative and full backing in their decisions. He claims not to know everything about the areas within his responsibility and therefore entrusts these areas to the people responsible for and qualified to handle them.

Mr. Kennedy himself described his usual practice as follows:

> One of my main philosophies in the matter is that I certainly let my people stub their toes. I'm not trying to be cute by that; I'm trying to help them learn. Of course, if what they make mistakes on is likely to have a serious impact on the laboratory I will step in, but I believe that these people are experienced enough in their own special field so that they will come to me if they need any help, and I don't have to be leaning over their shoulders all the time to make sure whether or not they are doing the right thing. Another thing that I have told them is: "Look, if you don't agree with me go see Arthur, and we'll settle it there." In other words, my word is not final at all, and they have disagreed with me in this way.

Mr. Kennedy also emphasized a philosophy of his which he illustrated by drawing a triangle, the sides of which were labeled "what," "when," and "how." He would then point out that he could only indicate the "what" of an issue for his people: the "how" and "when" were their responsibility and they had complete freedom in these matters.

Further data on Mr. Kennedy's philosophy of managing R & D is contained in Exhibit I, which is a paraphrased version of a talk by Mr. Kennedy before members of the American Management Association.

The views expressed by Mr. Kennedy in this talk may be placed more in context by noting that whenever the casewriters heard him express such ideas, either during a formal presentation or in conversation, the depth and sincerity of his emotional involvement in what he was saying was very evident. It should also be noted that these ideas were being expressed by a man who was not a working technical professional, and that it was apparent to the casewriters that the views he expressed were in total agreement with those of Dr. Kantrowitz. One final note about this talk concerns the observation by Mr. Kennedy that "One thing that always bothers, and yet amuses, me is that whenever I give such a talk people always come up to me afterwards and say 'Now, you don't really run your lab that way, do you?' "

Committee Chairman and Project Directors. Two research committee chairmen, other than the deputy director, were also interviewed. Both these men saw their roles as being limited to technical direction in which their main functions were to "keep the research in a given area moving in a proper direction" and to "act as spokesman for the director." These men carried out the former function by interacting with different senior staff scientists in ways that varied from man to man, depending on a man's needs and personality. For example, young, inexperienced scientists were given more technical direction and experimentalists might be informed of the theoretical approaches and results that might have affected their work. It appeared that the chairman could only indirectly influence the behavior of the other scientists in his committee and he did this only because of the respect given him as the most technically competent man in the eyes of these senior scientists.

One example of this kind of leadership was provided by a committee chairman who noted that one of the men commonly identified with his committee was doing work that had potential value to a related project, but that he was not communicating anything about his work to project people. He noted that he had been subject to some slight informal verbal pressure from the project on this situation. When asked about his response the chairman noted, after a long pause, that

> In all cases that I can think of, of this type of problem, I've thought about it and then left it alone. What I'd like to do is talk with the person involved and make some sense for him out of his communication with the project. In this way

I can inspire him to see the importance of such communication and therefore do it on his own. One thing you don't do here is pull rank.

The position of chairman, however, did give the incumbent two means of directly influencing the research at the laboratory. One means was his control over the agenda of committee meetings. Not every committee chairman used the agenda as a leadership tool, but one who did said: "You see, by leaving someone's work off the agenda I would be leaving it out of the mainstream of activity here, and eventually it would die." The scientists interviewed also believed that the consensus of opinion that developed at committee meetings and other informal meetings was taken "seriously" by the men whose work was being discussed. In this way the group of scientists capable of and interested in contributing to a man's work could guide that work. Another means by which committee chairmen might influence the research done at the laboratory was through their authority to review and revise all research reports written within the area of the committee's work.

One chairman also made several comments that showed the influence of Dr. Kantrowitz' characteristic traits on his role as committee chairman. It is important to note that these comments were made in a tone that clearly showed that the chairman was challenged by Dr. Kantrowitz' behavior. This man noted that both he and Dr. Kantrowitz had a good deal of fun "razzing" each other about the situations described below. The chairman started by describing one of Dr. Kantrowitz' characteristic modes of operation as follows:

CHAIRMAN: Well, one clue is this hit-and-run technique of his. At bull sessions he can often develop an intense interest in everything that's going on. In committee meetings, he contributes even if he is not very deeply involved in that particular area. But then again he will give great amounts of his energy to particular single areas for long lengths of time. He is often liable to get into an area in such a way that he can dominate the progress.

INTERVIEWER: Well, just what is the reaction of people around here to this?

CHAIRMAN: Well, in general this seems to have a favorable effect on people who are being hit. They get high priority. Everyone likes to have high priority certainly, because this means a great amount of privileges with regard to shop time, computing time, and other services. You see, we have no formal priority system here; therefore Dr. Kantrowitz' interests pretty well determine what priority system we do have.

However, the people who are bothered are frequently those who are not being emphasized. This is one thing that affects me constantly. You see, Arthur would like to be involved in new areas all the time, but in my area there are some bread-and-butter *old* problems that have to be solved if we are to make progress in these new frontiers. The men working on

these bread-and-butter areas get discouraged because Arthur doesn't show any interest in their work. As a matter of fact, he'll discourage these areas in public. He'll go out and make speeches and make comments that can frustrate people in these neglected areas quite a bit, and these comments do come back to the laboratory.

INTERVIEWER: What do you do in these situations?

CHAIRMAN: Well, I supply the leadership that Kantrowitz does not, and sometimes I try to supply interest and activity in something even to spite him. I try to show him that he was wrong in thinking that this was not a good area for research. My evaluation of this whole thing is that if we were ever to quit these areas, like he says he would like us to (but I don't really believe he wants us to), the laboratory would be in a bad way. I think he thinks so too, even though he wouldn't say it. But, you see, he believes that his function as lab director is to constantly be getting the laboratory into new areas.

The other major position at AERL involving management, or technical leadership, was that of *project director.* Most people described the differences between the job of project director and committee chairman in terms of the nature of their interaction with Dr. Kantrowitz and the extent of their respective responsibilities. For example, several people pointed out that Dr. Kantrowitz related to the work done in the committees through the individual senior research staff members. On the other hand, he related to the work done in the projects through the project director.

Another difference between the two jobs was noted by several informants: committee chairmen were primarily, and in some cases, exclusively, restricted to technical direction; project directors, on the other hand, had all the senior staff on that project responsible to them, and were therefore also responsible for costs, schedules, and customer relations.

Exhibit 1

A TALK BY MR. KENNEDY*

The romantic view is frequently taken that scientific research depends upon creative genius and the breath of inspiration, and that no amount of organization can contribute to the discovery of scientific principles. To counter this is the more realistic view that scientific research is 5 percent inspiration and 95 percent perspiration. The 5 percent of inspiration, obviously, cannot be organized. However, the application of informed management principles to the 95 percent of perspiration can either reduce the drudgery or increase the efficiency of experiments testing, fact collecting, and the expenditure of money and effort. In this way research might become much more effective.

When I speak of informed management principles, I refer to both the administration of a lab and its organization. Both should be compatible with the essential purposes of scientific research. The administration of a laboratory

* Paraphrased from a talk before a meeting of the American Management Association.

***Exhibit 1** (Continued)*

should, therefore, be an enabling process. This means that the function of the nonscientific member of the laboratory should be to provide the buildings, equipment, and services which the scientist needs. Here we must distinguish between administrator with a capital "A" and administrator with a small "a," even though the areas of finance, purchasing, personnel, etc., are very important to the smooth operation of a lab.

When we turn to the organization of a laboratory, the first important point is a differentiation between the typical organization in industry and the organization appropriate for a laboratory. The typical organization in industry, which is shaped like a pyramid, places employees in a work situation where they will tend to utilize few abilities beyond the less important, skin-surface abilities. In such a situation employees who are psychologically healthy will tend to experience frustration, as they do not obtain even minimum personality expression, and failure, as they see few possibilities for challenging work.

In response to the frustration, conflict, and failure, psychologically healthy employees react by such behavior as: absenteeism, turnover, transfer requests, increased mobility, trade union activity, emphasis upon money and de-emphasis of human rewards, goldbricking, rate setting, apathy, and indifference. Management, in its turn, reacts to these employee activities by strengthening its directive leadership, tightening controls, "selling" pseudo-human relations programs, and otherwise compounding the felony committed by the organizational structure.

In our laboratory the most precious single investment we have is people and the time they spend working there. And we organize our laboratory in order to enhance the value of this investment. In order to understand the way we do this you must understand the difference between two possible systems of authority. One type is executive authority which operates when the executive has ultimate control over top policy and subordinates work within limits dictated by this policy. The second type is colleague authority. Here the individual is supposed to set his own goals and choose his own areas of research and the authority rests with the man's group of professional colleagues. In our laboratory we have moved in the direction of the colleague authority system—the open or horizontal system. We feel this provides a truer alignment of the goals of individual staff members and the goals of the laboratory.

It is furthermore important that the organization of a laboratory not run counter to the basic nature of scientific investigation. This means, for one thing, that a research institution must not inhibit the self-motivating drive of scientists. A critical, introspective, nonconforming attitude is also the very essence of scientific innovation. Consequently, it should not be expected that the organization of a research group be similar to the organization found in other areas of business including the ordinary business administration techniques requiring organization charts, chains of command, etc. These two requirements can, however, be met if the research group is organized around an open or horizontal and flexible system of administration. In this system management must remain with its personnel at all levels to continuously show its desire to cross-fertilize ideas and promote a relaxed atmosphere of interaction.

Two final characteristics should be found in an organization devoted to research. The first concerns the observation that participation in decisions concerning their work and general policy is a motivating factor for professional employees. We believe that people support what they help create. We must, therefore, provide the innovator a sense of belonging to management. The second crucial point concerns the nature of the top man in charge. At AERL we

Exhibit 1 (Continued)

believe that the laboratory must be under the domain of a scientist, and I do not mean an ex-scientist, but one who plays an active role in the scientific endeavors of the laboratory; the philosophy of management and organization is then one which is conducive to research. Such an arrangement ensures that essential policy and management decisions will be made with considerable influence from the scientist and made to facilitate the work of the scientist. This does not mean that such decisions are made unmindful of the economic considerations. It does, however, provide for a totally different kind of management philosophy and one that does not conform to standard business administration.

Such an organization can not, however, exist on paper only, instead it must exist within the operational framework of an institution's contractual commitments in the particular scientific field in which this institution has special competence. At AERL we have oriented all our work toward the area of high-temperature gas dynamics.

Chapter 33 (Continued)

*AVCO-EVERETT RESEARCH LABORATORY (E)**

THE PRIMARY task at the Avco-Everett Research Laboratory was the discovery of knowledge about high-temperature gas phenomena: this was the "formal" work of the research staff scientists employed by AERL. Such work was conducted at AERL in much the same way as at any other research laboratory; activities included the speculative thoughts of individual scientists, the experimental investigation of natural phenomena, and cooperative speculation and argument between scientists.

However, AERL seemed to differ from many other laboratories in that the value placed on contact with working colleagues was unusually strong, and also because the theoretical definition of a colleague included a large share of the lab's employees. A particularly strong emphasis was placed on the status of Dr. Kantrowitz, the laboratory director, as *an active colleague* in the research work pursued at AERL. Dr. Kantrowitz himself strongly defended this definition of his role in his statements about the philosophy of management at AERL and through his heavy involvement in research projects.

The emphasis placed on colleague cooperation and cross-fertilization was demonstrated in many ways. It will be recalled, for example, that two of the most important values of the research group involved willingness to work in the broad area of high-temperature gas dynamics (chosen almost unanimously by the research group), and willingness to be freely communicative about one's work, regardless of the communicator's degree of confidence in the ideas that had resulted from his efforts.

An indication of the high acceptance given these values was that the researchers, as a group, did not believe that this value system had any negative effect on the technical aspects of their work. Only one case of such an effect was found [described in the Avco-Everett Research Laboratory (G) case] and this involved an individual who chose a technical direction which lessened his need for communication with his colleagues.

Cooperative working efforts at AERL took two forms. (1) *Informal conversations*, casual in nature, which usually took place in the lunchroom (there was only one for all levels of the staff) or in hallways or offices.

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These informal discussions were given important emphasis by most people interviewed. A number of scientists told the casewriters "We do most of our work around here in hallway seminars." (2) The other form of cooperative effort was the *formal group meeting*, of which the research committee meeting was the prime example and standard. The remainder of this case will focus on these committee meetings in general and on one meeting in particular.

The Committee Meeting in Theory

By far the most important influence on behavior at committee meetings, both in theory and in real life, was the scientific content of the then current research. The sole purpose of these meetings was to disseminate and clarify scientific data and information. It is therefore not surprising that the overwhelming majority of the comments made were technical in content. In fact, at least several people at AERL believed that no other information should be exchanged at these meetings short of truly important economic or strategic restraints on the scientific directions possible. This latter belief was particularly stressed with regard to personal matters. In other words, technical discussions were supposed to be purely objective and uncolored by personal feelings about other people. A man's comments were judged on their scientific merits: his status or personality were not legitimate subjects for discussion, nor were these factors supposed to influence judgments or critical reactions. On the other hand, many people also commented about the atmosphere which ordinarily prevailed at these meetings. Among the comments were frequent allusions to the "brutality" of technical criticism and references to the need to be "vociferous" or "violent" in order to influence the progress of a meeting.

Another important influence on committee meetings was the value people placed on individual initiative and responsibility. For example, it was expected that any scientist who was scheduled to present a paper would personally and almost exclusively handle the direction of the meeting if he wanted to present his paper according to a predetermined plan. However, it was also a primary purpose of the meeting to promote colleague interaction by allowing continued open comments from the floor. It was quite possible, therefore, for these ideals to be in conflict if the speaker believed a comment from the floor was a peripheral issue or an interruption in his planned presentation.

Another very important influence observed during these meetings stemmed from the behavior of Dr. Kantrowitz. His influence was expressed through the numerous technical comments and arguments he contributed and through the example he set of "proper" conduct in presenting such arguments. However, formal influence at these meetings was, in theory, negligible.

One formality that was observed, however, was the very practice of

dividing the research work into research committee *areas*, with each area having a *chairman*. Since committee meetings were convened only by area chairmen, it was necessary to have an idea recognized by one of the chairmen if it was to be placed on at least one committee's agenda. The superior status of the chairman theoretically ended with the preparation of this agenda, and during the meeting he was on a theoretical par with every other man present: that is, his comments were evaluated solely on the basis of their technical merit. The chairman did no more at these meetings than introduce each speaker, and he pointedly avoided any "head of the table" seating position.

In theory, therefore, the three strongest influences during these meetings were (1) the scientific nature of the research at the laboratory, (2) the value system of the research group, and (3) the personality, knowledge, and involvement of Dr. Kantrowitz. However, it should be noted that no one really seemed to believe that behavior during these meetings could or even should actually conform completely to this theoretical outline, even though the members of the research staff talked among themselves and to others as though theory and practice were identical.

A Record of the Events at One Meeting

In order to present a description of an actual committee meeting for comparison with the theoretical outline noted above, and in order to observe the interaction between the values shared by the scientists and the technical work done by them, one of the casewriters attended a committee meeting. This meeting was later described as typical in terms of its content, importance, and the atmosphere which prevailed. (It should be noted that the following record of the events which took place at this meeting represents the *impressions of the casewriter.*)

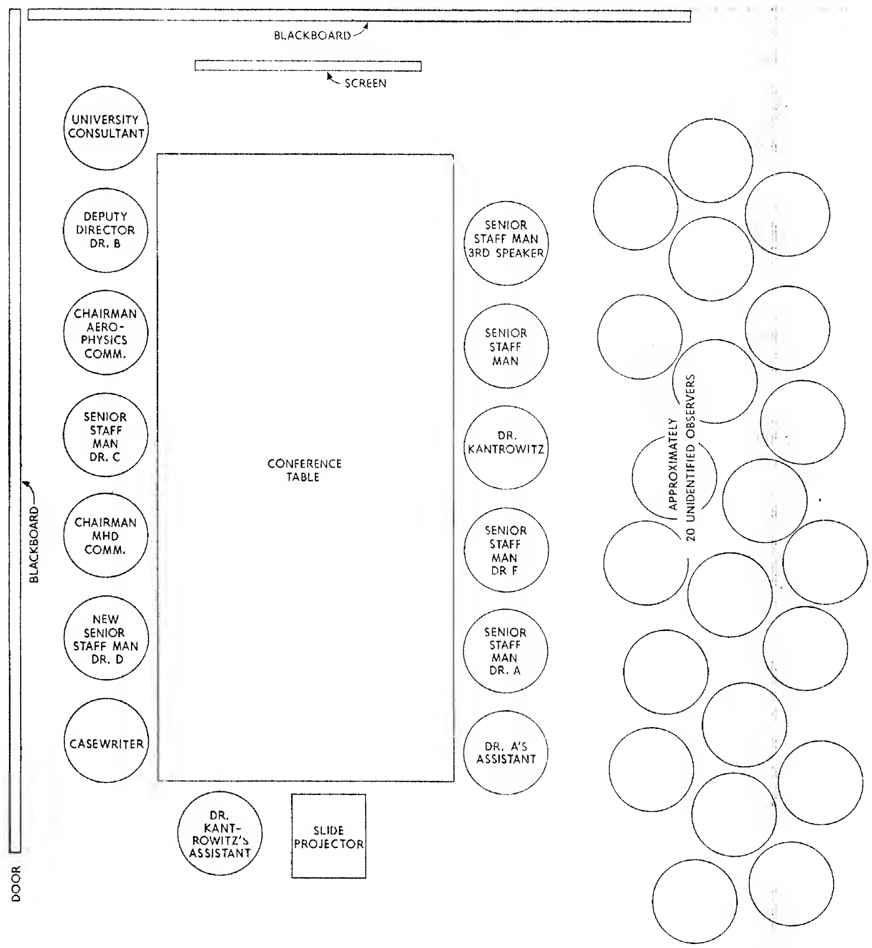
The meeting in many ways conformed very closely to the theoretical outline presented above. For example, the content of the meeting was almost exclusively scientific, with the major exceptions being laughter at technical jokes and the presentation style of the speakers. The chairman took almost no formal hand in directing the meeting and did not sit at the head of the table (a diagram of the seating arrangement is presented in Exhibit 1). The latter point, however, may be a moot one since one of the casewriter's first and lasting impressions was that most of the people at the meeting (including the main speakers) spent quite a bit of time *talking directly to Dr. Kantrowitz,* even though they were ostensibly addressing the audience as a whole. Dr. Kantrowitz, therefore, was obliged to be particularly alert at almost all times during a three-hour, very intensive discussion.

First Speaker. The first speaker (Dr. A.) opened his presentation with several graphs and a comment that was particularly interesting in light of

the way the norms about communication were expressed at AERL. Dr. A. started by pointing to an irregularity in his graphs and saying, "This wiggle certainly is interesting, and I'm not sure I know myself what it means, but I'm going to try to convince you anyway." The speaker made several other comments that pointed to the incomplete nature of his

Exhibit 1

SEATING ARRANGEMENT FOR THE COMMITTEE MEETING



experiments, results, and conclusions. Dr. A., nevertheless, forcefully presented and defended these admittedly incomplete ideas.

Dr. A. appeared to have developed a unique personal style of presentation for these meetings (he confirmed this during a later interview), which was designed to allow him to present his ideas in an organized way and to avoid sidetracks or premature discussions. Quite a bit of joking and

fun was generated around these efforts by Dr. A. to maintain control of the meeting.

Dr. A.'s effort to maintain control of the discussion was illustrated by the following incident, which involved the chairman of the committee. The chairman started an exchange by saying to Dr. A.: "Really, what you are saying is somewhat redundant," and proceeded to illustrate his point with a long (five to ten minute) talk with several blackboard notations and conceptual diagrams. After Dr. A. had made an answer, the chairman tried to go on with his argument but the speaker interrupted by saying: "Let's go on now. Let's not belabor this thing." The chairman was not satisfied, however, and made another technical point to which Dr. A. replied: "Yes, but you haven't seen the kicker in this thing yet. Let me go on and explain this further." A short time later the chairman made another point, apparently looking for an explanation of his own argument. Dr. A. agreed to this point but told the projector operator to go on to the next slide, on which he then commented.

One other mechanism for controlling the course of the meeting was observed. This centered around the constant interruptions of people by one another. Several times two or three people would speak at the same time. When this happened the man who was shouting the loudest would hold the meeting's attention.

One of the variables in the theoretical outline of what committee meetings should be like was the ideal that all people present, regardless of their positions, were free to make comments during a meeting. The committee meeting attended, however, appeared to indicate that this ideal was qualified somewhat since only those who were actively engaged in work similar to that being discussed actually made comments. The meeting's secretary explained the limited participation of people at the meeting in the following way: "Only those who are interested and experienced in the subject area as a part of their own work are going to participate. If you came to other meetings you would find that the participating group changes from meeting to meeting."

The men who did participate (made more than one comment) were: Dr. Kantrowitz, Dr. A., Dr. B., the committee chairman, Dr. C., Dr. D., and Dr. F. Only two men who were not seated at the main table made any comments, and these men worked with Dr. A. on the work being discussed and made their comments only when questions were directed to them.

The participation of Dr. D. was particularly interesting in light of the fact that he had only recently been hired as a member of the senior staff. Dr. D. had, however, done his graduate work in the very area being discussed by Dr. A. Dr. D. tried several times to make a point: once he was overruled by a question to the speaker from Dr. Kantrowitz, and, when he spoke a second time, the chairman turned around (he was sitting in an adjacent seat), looked disparagingly at the young man, and made a short

remark which appeared to dismiss Dr. D.'s point. The young scientist's reply was to hunch his shoulders somewhat and say in a rather quiet voice, "I'm sorry." Later, this scientist reiterated his argument, and, after some striving to be heard, his comment was accepted as relevant.

The meeting also conformed closely to theories stating that the speaker was supposed to be the man responsible for obtaining and interpreting useful information from the discussions pursued. Thus, although several people made technical comments, only three men made any comments which either related to the future directions the research could take, or summarized and related the points made by other speakers, or tied the subject into the broader framework of the technical goals of the laboratory as a whole. These men were Dr. A., Dr. B., and Dr. Kantrowitz. For example, at the close of the first discussion, Dr. A. summarized, and related all the technical points he believed relevant, and ended: "You think it's a long tough road, and it's going to be difficult to get some really firm results on this. Now this doesn't matter much because we certainly are going to go on anyway, because we have momentum now."

Another example of the operation of this value of individual responsibility concerned the fact that not only was the speaker responsible for what good he got from the meeting but so were all others present. Mr. Kennedy had illustrated this point by telling the casewriter that if a man wanted to sleep through such a meeting, no one else would mind in the least—although they might make a few mild jokes. Indeed, one man, Dr. F., did sleep through most of the two presentations attended by the casewriter, and no one appeared to take any notice of this.

Second Speaker. The humorous content of this talk was high, a considerable amount of laughter being generated around the presentation style of the speaker. It seemed that those attending the meeting were less confident of catching this speaker in a mistake, although they tried to do this often. For example, Dr. B. (the speaker and deputy director of the laboratory), in answer to some technical points made by a senior staff member, said:

DR. B.: Yes, but you see I'm really cheating on you. I'm leaving out a lot of detail, and there are very many details here. I spent two weeks working them out and narrowing down the core of this. So I bet I can beat you 10 to 1 on these details.

KANTROWITZ: You're using small "l" and capital "L" interchangeably.

DR. B.: Ack! (Explains why he made the mistake)

KANTROWITZ: (Smiles)

DR. B.: (Presents his conclusion)

KANTROWITZ: Oh!

DR. B.: You're going to tell me what's wrong.

KANTROWITZ: Yes, I think so. You seem to have forgotten "x."

Dr. B.: No, I didn't; I purposely avoided it all along. (General laughter)

Kantrowitz: (Laughingly) You're a hard guy to get along with!

This second talk seemed particularly important to Dr. A. because Dr. B. was presenting some theoretical results that seemed to be at variance with the experimental results achieved by Dr. A. Dr. Kantrowitz pointedly steered the discussion (after Dr. B. finished) by asking for Dr. A.'s "rebuttal."

As Dr. A. made his rebuttal and Dr. B. his counter arguments, the casewriter noted that the discussion was getting more and more heated. Dr. Kantrowitz also seemed aware of this for he tried several times to reconcile the differences between the two men and to tone down the emotional content of the discussion. At times Drs. A. and B. also attempted to tone down their own comments. Dr. A., however, also tried to settle the discussion by trying to enlist the support of others, particularly Drs. D. and F. Dr. A. did this by asking these men questions about their own work and at times by talking to them while Dr. B. was presenting a point. Through the efforts of all present a tenuous calm appeared to develop but this was broken when the committee chairman interrupted one of Dr. A.'s comments with a negative remark. This interruption seemed to raise the emotional level of the discussion, and Dr. Kantrowitz broke in to say: "Now how about moving on to the next item on the agenda? This should be settled privately." The chairman, however, pressed ahead, speaking about the ease of testing Dr. B.'s results by experiment. Several speakers, including Drs. A. and B., then made some calming comments. The next speaker also spoke in a calm, relaxed manner, and on a different topic than the first two speakers.

Post-Meeting Data. Dr. Kantrowitz' assistant provided some data on this meeting, as follows:

Interviewer: I'm very interested in the fact that many people told me that there are never any personal feelings around here. Yet it seemed to me that at that meeting there was a considerable amount of emotion involved.

Staff Member: The feelings that I saw expressed yesterday were not. You see, Dr. A. could not draw from that particular discussion the idea that he was a good or bad experimentalist. What conclusion he would draw would be to say, "Well, I haven't proven my point yet."

In other words, in other organizations you find that two guys get really mad at each other and they won't talk to each other. Their attitude is one of "Well, put your dukes up; I'm ready to smash you in the face." You don't get that type of attitude here at all. As a result of meetings such as we had yesterday, the personal relations are just as good afterwards as they were before.

Now one might have also concluded that Dr. A. should terminate the

work he's doing right now, but if he feels that what he's doing is important, he's given an opportunity here to prove it. He's also expected to come back and keep people in touch with what he's doing. Around here no one ever says: "I'm the authority on this job, and you will stop doing that or go this way." The authority here is the man himself, and it's up to him to demonstrate the logic of what he's doing.

Dr. A. also discussed this meeting at a later date, and in his discussion of it he made several sincere remarks about his high personal and professional regard for Dr. B. He also noted:

Dr. A.: Now it seems to me that it's a compliment here when other people understand what you're saying and when they try to be a part of it. They understand it well enough so that they try to do your own job for you.

Interviewer: How does this contrast with the views I have heard from other people here that, since you are intimately connected with your work on a day-to-day level, most other people cannot possibly know the details of your work and would not be able to be of much help?

Dr. A.: I don't believe that at all. You can be too close to your work; and one of the reasons that this laboratory has been successful is that it has provided a place in which people that are not too close to each other's work can contribute to each other's work. That meeting was wonderful for me because I decided to take a different attack. In a way, though, it's a mixture of humility, being willing to take other people's viewpoints, and pride in your own work. You just kind of let it soak in. I suppose, though, if you are frustrated, that you can feel this other way.

Finally, the reaction of the new, young scientist whose contribution was several times ignored and then accepted: "At the time that I made my comments I didn't think that they were received at all. In the first place, I guess I tended to understate them, but then again, the next day Dr. A. came up to me and told me that he was going to do some of his research my way, and that made me feel good."

Chapter 33 (Continued)

AVCO-EVERETT RESEARCH LABORATORY (F)*

IF AN optimum atmosphere for personal and professional growth may be considered a worthwhile goal for a modern research organization, then the nature of this atmosphere at a laboratory noted for both its productivity and managerial ability should be of particular interest to students of management. Further, if the working environment at the Avco-Everett Research Laboratory was as strongly demanding of individual maturity as many people claimed it was, then an understanding of how such a mature work force had developed at AERL would also be important.

It has been noted that recruiting and hiring practices tended to legislate against new people who would not be likely to measure up to the standards and values which had evolved as a working code at AERL. [See Avco-Everett Research Laboratory (A) and (B)]. The recruiters looked for men who not only had technical skills which were well-recommended, but who also appeared to come to the laboratory with built-in motivation (in the right direction), a high degree of initiative, and considerable self-confidence.

Commenting about the demands made on new men, some of the older researchers made such remarks as:

> There's a tendency not to respect a person unless he's an expert in his own field. We want new PH.D.'s, but they are supposed to know enough to solve all the problems we've been working on for years.
>
> To some extent our laxity of management is expensive: you can hang yourself around here real good. A corollary of this is that the research people you hire have to be real good or else you'll make a real bongle of the whole thing.
>
> New people coming in here flounder. They don't get specific assignments and they tend to be rather bewildered by this, at least until they learn to take initiative for themselves, which is the system on which the laboratory is built.

The casewriters wondered how new recruits responded to these demands. They interviewed a number of them and discovered, first of all, that they tended to be highly motivated in joining the laboratory staff. Their reasons for coming to AERL, instead of another laboratory, were:

1. AERL has a very high technical reputation: it has the acknowledged leaders in the field.

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2. The consulting arrangements with some of the most prominent names in science.
3. Plasma physics is such a young field that there are not many places in the world where you can get five good people together. If you go somewhere else, that doesn't have good people to talk to, you are at a real disadvantage and pretty much on your own. I didn't want to fumble around on my own.

By reputation, then, AERL was known to these young scientists as "the best," and many of them, while needing a period of adjustment to the demands of the working code, appeared to accept, at least in theory, the requirements and standards they found operating at the laboratory. Many recruits, in fact, did not perceive the working atmosphere as overly demanding; they had been trained to expect criticism of their work, for instance. One new senior staff member had this to say about the meeting at which he had been asked to talk about his work before being hired:

It certainly was not brutal, and it was not an imposition in any way. Any criticism that was raised was very valid criticism. I personally believe that anyone doing technical work should expect to be criticized technically, and, in fact, he should seek out such criticism. It's a chief factor in his advancement in technical areas. I also don't particularly care on what level you are working or who you are working for: I think you need criticism. It's really quite important, and many organizations don't have it. I know that at my last job we certainly didn't have any criticism, and it was a very important disadvantage there.

Almost every person who had worked at the laboratory for any length of time appeared to have developed strong feelings about how people at the laboratory should feel and behave in relation to their work. One of the senior project staff memebers verbalized his beliefs in this area as follows:

To my way of thinking this whole thing they call motivation is the most important thing. What I mean by this is that the people here are people who get their satisfactions out of *doing* things. Another type are people who get their satisfaction out of *being* somebody. In other words, when I see a guy who likes titles, like Mister, Vice President, or Department Head, and flashes these around, then I know he is just not the type of person I can understand. And I'm not at all on easy terms with these types of people.

The other types seem to find their security within themselves. I guess in a sense you want to continually test yourself. You want to know can you go to the extreme, and I get a lot of satisfaction in this job from this type of thing. You know around here you really dive into your work as though your life depended upon it. For example, there was one project during which we made a very simple procedural mistake and lost a few weeks of data, and I literally sat down and cried like a baby. I would also say it is not so much a question of how you develop this type of attitude in people as how you maintain it once you have it. And as far as I am concerned it is not a question of knowing what rules are the important ones, and so on. It is rather one of getting a perspective on people.

I also have a philosophy of my own about people, and that is that there are some people who want to settle things. They want to pass a law when something is wrong and they want to have it all wrapped up in a neat package. You do this; you don't do this. We know from history, I guess, that this doesn't really settle anything or really help people to grow. The way I feel about this is that there

are always many, many different checks and balances in all phases of life so that any time you set about to improve something you'd better do it judiciously, because what you do is going to affect the other phases.

While talking with this man and others who expressed themselves in similar fashion, it appeared to the casewriters that the personal adjustment expected of the men working at AERL was not to be built on a surface surrender to the demands he faced at the laboratory, but rather on a deep and individual incorporation of the values underlying the working atmosphere. The system tended, it seemed, to be self-supporting since the men who were hired were believed to be those who believed deeply in the functionality of these values.

The casewriters thought it might be interesting to test the degree to which the senior men felt it advisable or necessary to help new men adjust to the requirements of the system. Some evidence which bears on this question was obtained from two senior staff men:

STAFF MEMBER: When I first came here I had a lot of problems in adjusting to this environment. Now there are some people that have worked here that have left—and by the way, they did a good job after they left; they just couldn't take the free and easy give-and-take that we have here.

INTERVIEWER: Was there anything here that helped you get over this problem?

STAFF MEMBER: No, I just didn't want to quit here without accomplishing something. In order to make this adjustment I think that what you have to do is fight very hard against self-pity. You have to look at yourself as others are likely to see you, and accept other people for what they are.

INTERVIEWER: In other words, it is entirely up to the individual to work out his relationships with this environment?

STAFF MEMBER: Certainly! Very definitely! But I'd like to note that if you believe you are objectively doing a first-rate job, and other people are unwilling to admit this, after a while you are just as well off to leave. Then you are at peace with yourself, and you can leave under those conditions.

Another man, a senior project staff member, also discussed the basis for his ability to work at AERL. He started by noting that he believed that a flat organization, like the one at AERL, allowed the individual room for growth—it did not put upper bounds on him—whereas a hierarchical organization did put upper bounds on its people and thereby limited their room for growth. This man went on to note that once an organization eliminated such bounds it was up to the individual to take care of his own growth. He then discussed his personal philosophy regarding his work.

STAFF MEMBER: I am here to be useful to the research scientists, and as long as I'm not intrigued by my own importance, then I'll be okay. But

once I look down on the PH.D., who is really doing the work here, I'm going to get into trouble.

INTERVIEWER: How do you develop this attitude towards your work?

STAFF MEMBER: Well, I think you have to have an attitude towards asking people "Let me understand you and your work so that I can help." Then I think that you have to work on your own and depend upon your own doing. You also don't want to sit around at your desk and write memos; you want to get up and talk, talk, talk a lot more than you sit.

Both these men also discussed the question of how they helped people who were not adjusting well to the atmosphere at AERL. For example, the senior research staff man made the following comments:

STAFF MEMBER: Some people are more sensitive, and I feel sorry for these people here. I try to, or at least I wish I could, help them. You can tell who these people are pretty easily.

INTERVIEWER: How do you go about helping these people?

STAFF MEMBER: I'm not quite sure. I guess you start by making a judgment on whether you like the person. (Pause) Oh, I like everybody here! I guess you try to be a little less critical of these people, and after a while, when their confidence builds up, you can treat them as other people in the laboratory.

One of the problems I see is that in science you get certain people who you know have been getting a lot of good grades and a lot of approbation from people all along the line. It's awfully hard under these conditions not to get an awful high-blown opinion of yourself. If these guys then go to an environment where others are equally sharp, it is very difficult for them to make an adjustment, to accept criticism, and to just be one of the boys and not have such a high-blown image.

You can sense the insecurity in these people. What they need is experience working with equals, as we all are here at AERL.

The senior project staff man took a different approach to helping people adjust.

STAFF MEMBER: Well, we made a decision around here to hire good people, and if they don't work out we just get rid of them. You see, around here the senior staff man is the man who can be self-supporting. However, one of the other interesting things is that some people adjust real well to this environment and others don't.

INTERVIEWER: Is there anything you can do to help those who don't adjust too well?

STAFF MEMBER: Damned little! Now what you start out with around here is the assumption that people are old enough and mature enough to work in this environment. As far as I know there's no organized way of

helping such a person if he's having problems. Instead, you have to work by giving individual-type help. Someone has to spend several hours talking to him and explaining how things work around here.

INTERVIEWER: In other words, you tell him what the laboratory situation is in these talks?

STAFF MEMBER: That's right. It's the same type of thing at any other company. The first thing you do when hired is get an organization chart, but you very shortly find out that this doesn't represent the key men; that there is a phantom organization. You then have to spend some time in determining what this phantom organization is if you are going to get things done. So you tell people what this phantom organization is: you straighten them out.

In other words, you sit down and you explain to a guy what his function is at the laboratory and you tell him that here's the way things work here, here are the rules, here are the people who are really proficient in such and such an area. You also tell him little tricks, such as if you want to get something purchased, the best way to do it is to hand-carry the requisition down to the purchasing department.

This informal and unstructured system for helping new men was characteristic of all developmental activities of the laboratory. As in every other area, the growth of the individual was left pretty much up to him. All men were given room for self-development; those who could not grow soon found themselves left behind, supplanted, or bypassed. This "fact of life" was apparent not only among the research staff but also in the project groups.

The casewriter became acquainted with this informal system when he was told that one of the senior staff in one of the projects had been turning in a somewhat discouraging performance. Two things had happened in this case: first, other senior staff men had voted at group meetings to take specific tasks, like writing a report or delivering a speech, out of this man's hands. Second, there had been a "natural filtering" of new and old responsibilities to other people on the project. One man explained the process in operation by noting that as a new area arose someone would say "Oh, Adam should be formally handling this. He's been doing a good job on the side so far."

In other words, as in so many other areas at AERL, if a man failed to take initiative himself, he might find himself with a smaller and smaller share of responsibilities for project management. In the particular case described above, this process seemed to have gone quite far since one other man on this project, in describing his duties as he saw them, was actually describing the duties that were formally assigned to the man having difficulty.

Another group concerned with growth and advancement at AERL were the technical people who did not hold senior staff rank. One member

of the senior research staff described the road for advancement and the obstacles faced by these people as follows:

You see there is a clear-cut differentiation here into two types: those who are capable of going on to doctoral work, and being senior staff-type material, and those who are not. Now one thing about these two types is that it is like black and white. In other words, is is very obvious who they are. One of the things that I try to do with the first type of person is to go to bat for them and try to get them fellowships, sponsored by the laboratory, when I am pretty sure of their capabilities. Having these people around here is very good for us, too, because it keeps us constantly stimulated by new people who are young. It is kind of good for your psychological health.

However, with the second type the *esprit de corps* is rather hard to maintain. The only long-term advice that I can give these men is to tell them that "I'll try to help you in any way to change your classification while you are here at the laboratory. However, if you want to grow, then probably the best avenue is to grow through managerial experience. However, for engineers this is not a laboratory for this type of growth. It would be better to go to one of the other defense-type laboratories where you can get considerable managerial experience in managing technical programs." Most of them eventually take this avenue.

The problem is that we might have a man who can build the best darned shock tubes around. Well, there just isn't enough market for that type of thing to mass produce them. Although such a man is valuable to us, once he has mastered this technique there isn't much avenue for him to grow, so the best step for him would probably be to leave this laboratory.

Chapter 33 (*Continued*)

*AVCO-EVERETT RESEARCH LABORATORY (G)**

DURING THE time the casewriters were active at the Avco-Everett Research Laboratory (AERL) three instances of conflict, one between groups, one between individuals, and one between an individual and the social system, were brought to their attention. This data was particularly interesting in light of the persistence of a number of people at AERL in denying the presence of any conflict at the laboratory. For example, one man pictured an organization in which there was no personal tension, in which decisions were always unanimous, and in which people adjusted to each other with no difficulty. This description prompted the casewriter to ask:

INTERVIEWER: I'm very interested in what you say because, according to many people, there are no "politics" here at the laboratory, there are no hurt personal feelings here, and there are no cliques here. Furthermore, you're saying that you always reach unanimous decisions, and that you accommodate each other without getting squashed. I wonder about this because certainly, even in marriage, you have squabbles every once in a while.

STAFF MEMBER: Well, I've never had a squabble in my marriage. (Pause) Well, I guess you can conduct squabbles in different ways here. This is due to the fact that it's impossible to ever beat a man down here, so the only thing left for you to do is to convince him. You see you can't hit him, you can't boss him, you can't fight him, so what *can* you do?

INTERVIEWER: What if he won't be convinced?

STAFF MEMBER: Well, you have to remember that at this time you are also undergoing a change, and perhaps even a third party has come along to straighten you both out with a clear point of view. The thing to remember is that you just can't lose by being wrong around here. You are at liberty to change your mind as frequently as you change your shirt. It's kind of like a knight's battle with cooked salami. You can, of course, jump all over someone, technically, but you never really draw any blood. A man may go home with a lower lip stuck out, but he's certainly not making any less money.

At the same time, however, it is interesting that no one around here

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ever gave me the "itch," and that's certainly odd, and I don't know why that should be. I can't say that all the habits of my colleagues are pleasant. Some of them have the manners of a wart hog, but somehow that doesn't bother me.

As noted, it was clear from other interviews that there was at least some conflict within the organization. On the assumption that this was to be expected, and that methods of handling inevitable conflicts were of particular interest, the casewriters decided to explore further.

Conflict between an Individual and the Social System

The first instance of conflict concerned a member of the senior research staff who was not, in the eyes of many people, doing an adequate job. (One method of handling such situations within the project groups was described in AERL [F].) In the project groups an individual was allowed to work himself into an area of less and less responsibility if he did not take the initiative. As we have seen, however, the norms in the research area stressed individual rather than coordinated activity and the research committees were also more stable in terms of growth. With these differences in mind the following comments, made by the man having difficulty, are particularly interesting as an illustration of how *he* perceived the social organization of the laboratory:

STAFF MEMBER: I don't think committee meetings are too helpful. Since I'm accustomed to academic life, I tend to think you get more done by keeping to yourself. But there certainly are reasonable reasons for having such a system as far as the lab as a whole is concerned.

However, I don't believe that you can get effective help with your individual work at these meetings. I also don't believe that any real science is accomplished at them, even though you do get this cross-fertilization that the lab wants. The only thing these meetings show is that people here wouldn't talk to each other without them.

One of the problems, I guess, is that originally I worked with a committee chairman and was kind of isolated from the rest of the lab. This, and the fact that I am a theorist, explain why I wasn't originally aware of the emphasis on communication here. This close association I originally had was also very much like the tutorial system you find in universities. With this system a young man is closely associated with and works under an older and more prominent man. Now I don't necessarily believe that this is the system I want for myself, but it can be a rather inspiring relationship.

You see, a theorist doesn't object to communicating the results of his work but he wants it to be pretty well digested first. At that time criticism is fine and welcome. I'm not really too unhappy with this system here; the only difficulty is this difference in the interpretation of what is digested

enough to communicate. But no one is really pushing me to communicate. You see, I've been kind of off on my own on an approach that has been kind of peripheral to the activity here at the laboratory, and I really only spoke on this once. Now I am working on something that is also somewhat peripheral. I know I am risking my salary and my job in doing this, but these areas are my theoretical interest right now.

INTERVIEWER: Does this threat to your position bother you in any way?

STAFF MEMBER: Actually I just react the other way. That's another reason, for instance, why this committee system won't work. When you threaten a person, they'll fight back and react against the threat.

Laboratory management reacted in these terms to this situation:

INTERVIEWEE: Well, as far as I am concerned, this man just doesn't belong in this laboratory. He seems to need someone like Dr. Teller, who understands his area. He wants to work with big names like this, but he doesn't seem to want to work under Dr. Kantrowitz or his committee chairman. However, he is a very brilliant fellow.

INTERVIEWER: What has been done to alleviate this problem?

INTERVIEWEE: Well, this has gone on for a long time and finally we have told him that we were unhappy with his performance and that he had better change or it would be best to look for other opportunities.

Dr. Kantrowitz had been a senior thesis advisor (undergraduate school) for the man having difficulty. When asked about the conflict that had resulted at AERL, Dr. Kantrowitz repeated the analysis quoted above. When the casewriter asked Dr. Kantrowitz where he had received the evidence on which to base this analysis, Dr. Kantrowitz mentioned his years of association with the man in school and at the laboratory. The scientist involved, however, noted that Kantrowitz had not actually had a very close association with him either at the lab or at Cornell.

While a doctoral student, this scientist had spent his summers working at AERL and had also done his thesis as part of an AERL research project. This close association had apparently been a very satisfactory one for all concerned, and AERL's management believed this man could develop into an important addition to their staff.

One senior staff man mentioned a factor that might have had an influence on the way the situation had developed. This man had just finished telling the casewriter that AERL was similar to other labs in that once a man was branded as "nonsenior staff" it was very difficult to break the stereotype. The interviewee then stopped suddenly and wondered if this tendency to stereotype might not have had something to do with the conflict centering around the senior staff man involved in the instance just described. He said that since this scientist had worked for a long time on a special basis, but not as a senior staff man, it might have been difficult for

him, and others around him, to make the adjustment to the different and strong demands and expectations that were involved in the role of a senior staff man.

At the end of the casewriting period the scientist concerned decided to leave AERL for a position at a laboratory associated with a university. In his new job he would be working in the technical area (not high-temperature gas dynamics) that had been of primary interest to him. Before he left AERL, he made the following comments:

> What I said before, about no one pushing me to communicate, changed, and this precipitated my decision to leave the lab. I was pushed on communicating more and this took the form of a "man-to-man" talk with Dr. Kantrowitz. Dr. Kantrowitz tried to convince me to speak up more by telling me how hard life was, that a scientist had to be able to stand up to the technical critiques of his colleagues, and how important it was for a scientist to contribute to the community he belonged to. He also noted that the only way he had of administering the lab was through the communications of the staff here.
>
> Now I told him that I didn't mind communicating on my technical work; after all, I don't believe that anyone can do significant work entirely on his own. However, while I can be enthusiastic about the work I am doing, I believe very strongly in not being boastful. I talked quite a lot about modesty to Dr. Kantrowitz and told him it was not in character for me to be boastful about my work or knowledge. I also don't like the administrative motives behind some of the demands for communication here.
>
> I think people here also believe that I only want to work for big names. Now this isn't quite true because actually I believe that many of these big names have built their reputations on oratory rather than ability. But what can you do to make people here see that it is possible to think differently than they do?

Conflict between Individuals at AERL

Another conflict situation at AERL had existed at one time between two members of the senior project staff. In this instance a customer of the laboratory decided that a critical research question needed answering before they would commit any further major funds to the project. This question could only be answered by an all-out effort on the part of the project group. However, one of the senior members of the project staff (in charge of the key piece of experimental equipment) tended to identify himself as a researcher and balked at putting himself under the project director and abandoning his own work just to answer this question.

The situation between the researcher and the project director became rather tense and it appeared possible that this important researcher would leave the lab. The situation was therefore an important one (the possibility of losing a key man and the policy issues involved in the situation). Top management's approach to this situation was to have several "conversations" with the researcher and project director, but to refuse to enter the conflict as a "referee." Management's attitude was described by one man as follows:

For a time this situation was a real upheaval, and it seemed that Arthur would have to decide whether he was going to let the research philosophy continue in the projects or change this philosophy. However, Arthur didn't want to work this out dogmatically, so that there would be a point where we would be assigning senior staff to projects or research. Therefore Arthur insisted that the project director talk to the scientist and that together they work it out.

This attitude was maintained even though the situation had been foreseen and had built up over a period of several months. The conflict eventually became resolved when the researcher decided to work on the customer's problem and agreed to a definition of the senior project staff man's job which stated that the project director had authority over the senior staff on his own project.

Group Conflict at AERL

At the first meeting between the casewriters and James Kennedy, he mentioned that several problems had arisen around the technical service departments. Kennedy asked the casewriters if they knew of any literature on the management of service groups. Later the casewriters heard from several people at the laboratory about conflicts between projects and committees competing for the limited amount of service capacity available. Other problems that centered in the technical service groups concerned the nature of the work done in these groups and the question of whether separate service facilities should exist within the larger projects. A senior staff man in charge of one of the technical service departments discussed several aspects of the former issue:

> The problem concerns the interface between the projects and the service groups. For example, one way of looking at this interface is that the projects think out a matter in detail before they come to us so that they can tell us exactly what they need, and then all we have to do is run it off. The other way of looking at this interface is that the projects tell us about the function they want fulfilled and then we have to design the thing that will fill this function, as well as make it. Now in general this interface had been moving towards the project groups; in other words, things have not been thought through in detail before they were brought to us. Recently, however, one large project has wanted to move the interface towards the service groups, taking on more and more design activities themselves.

Apparently, the decrease in design work in the service groups was causing some of the people working in these groups to look on this interface shift as a downgrading of their importance at the laboratory. They saw their groups as being given more and more routine work while the professional and challenging work remained in the project groups. They were furthermore concerned that the nature of the routine work would give them little more to do than to fill the orders of the projects.

This situation was further complicated by the fact that senior project engineers appeared to believe that only routine work was needed in the

technical services. These senior project engineers therefore saw no need for consultation on a senior level with some services. Some technical service people, however, resented having junior engineers telling their senior people what to do. The problem was compounded by the emphasis on "service" in the organizational role of the service groups, which made some service people feel helpless in discussing these conflicts with others at the laboratory. Another belief of some service people was that some junior engineers in the projects were too stubborn to see the need for design changes by the technical service staff. These problems were generalized in at least one technical service group in which some people believed that the rest of the laboratory lacked confidence in them and were not aware of the difficulties involved in carrying out their jobs.

The other major problem, centering in the technical service groups, concerned the establishment of separate technical services for the project groups as the projects became larger and more autonomous. The need for these separate service groups was related to two unwritten policies at AERL: one which stated that technical services were organized primarily to serve the research staff, and another which stated that projects would be split off from the lab as they became strong enough to stand on their own. Apparently, all the projects were at least in the first stages of such a splitting-off. The issue was, however, complicated by the fact that some of the project staffs believed they were not being adequately serviced by some of the technical service groups. For example, a senior project staff man noted that one service group suffered from a lack of organization and poor coordination with the purchasing department.

One of the senior technical service staff men was also troubled by the establishment of separate technical services for the projects even though he recognized the eventual need for such groups. This man had suffered a 50 percent cut in the workload of his group as a result of the loss of work which resulted when two major projects had established their own service groups. This cut had caused significant morale problems in his group. He had eventually solved his problem by getting the project concerned to "hire" some of his men.

The problems this service group faced were further complicated by an apparent difference in values between the senior staff man in charge of the group and the value system of the laboratory. For example, the service group leader made the following comments about the atmosphere at the laboratory:

> This question of freedom sure is very basic with me. Just look at the nation: freedom of speech we have, sure, but certainly within limits, and you'd better not step out of those limits. I think to talk about freedom in any other way is foolish. Actually, what you do is foster irresponsibility because no one stands in judgment over this freedom or evaluates the work. You see, there's no strong direction by management here to see how every dollar should be spent, and I believe certainly that there should be because when something goes wrong

around here now, you just can't blame anybody. I think that this control is pretty important in order to rectify the situation and make sure that it won't happen again. But around here what happens is that you can cover up a lot with this unbounded freedom.

One senior staff member, on hearing that the casewriter had just talked to the technical service manager quoted above, gave his viewpoint on this man's problems. He believed that the service manager felt that James Kennedy should resolve his problems. The staff man then noted that most of the people around the laboratory believed that it was up to the service manager himself to take the initiative on any problems that he had. He added that most people believed that the initiative had finally been taken, but that it had not been taken until the situation had almost worked itself out.

This staff member also believed that the service manager's complaints were due in part to repeated requests for technical service people to transfer into the project areas. The staff man felt that the manager honestly didn't believe that many of his people were qualified for the job they were being requested for: for instance, senior research designer. The staff member noted that a couple of months after such a transfer the project people would say: "Boy, he is the best senior designer you would ever find; in fact he's not even a designer—he's almost a real professional engineer." As a consequence this staff man believed that people in this service manager's group were getting the impression that they were being held back. Both the staff member and top management at the laboratory summarized the problem by noting that the situation was caused by "personality problems."

The casewriter wondered if part of the answer to some of the difficulties in this area could be found in the attitude the technical service manager attributed to himself and others in this comment: "You know, I wouldn't tell Jim Kennedy some of the things I've told you because it wouldn't do any good. He wouldn't really pay any attention to what I said."

Summary

One senior administrative staff man summarized management's attitude toward conflict resolution at the lab. His comments were particularly interesting in the light of comments by other people who had noted that the people at the laboratory tried hard to avoid being "referees" in any conflicts. This man said:

Kantrowitz will not attack any of these issues until they have developed into very serious problems. This, in a way, is one of the prices the laboratory is paying for the advantages it is getting through its system. In other words, a man having difficulties fitting in at the lab might be able to be helped out of them, or might be able to be transferred into a situation where his problem was less criti-

cal. However, by letting the situation go and develop on its own, we hope it will eventually come to a point where it will work itself out naturally through a man's own efforts or else through natural events. We know this is risky and could lead to serious difficulties that might be avoided, but we are willing to take this risk.

On the other hand, Jim Kennedy had noted that even though it took management a long time (measured in years) to be sure that an individual was doing badly, once this was determined a man would be quickly let go or demoted. One such situation arose during the casewriting period and the senior staff man involved was, within a week, demoted to a lesser job.

INDEX TO CASES

INDEX TO READINGS

This book has been set on the Linotype in 10 point Janson, leaded 2 points and 9 point Janson, leaded 1 point. Chapter numbers are in 18 point Mono Janson, c. & l.c. italics with arabic figures and chapter titles are in 14 point Mono Janson italic caps. The size of the type page is 27 by 46½ picas.